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PHENOMENAL REGRESSION TO THE REAL OBJECT. I.

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I. THE APPARENT SHAPES OF FIGURES OBSERVED OBLIQUELY.

IT is commonly stated in textbooks of psychology that when we observe figures inclined to us, we see them not in the shapes indicated by the laws of perspective but in the shapes which these figures 'really' possess. Thus when we look obliquely at a circular object, we see it not as an ellipse but as a true circle[1]. While it is undoubtedly true that such an object seen in these conditions is judged to be of its true shape and also that we are prepared for motor reaction to a circular object, I do not find that experiment confirms this statement as to what shape is seen. If a subject is shown an inclined circle and is asked to select from a number of figures the one which represents the shape seen by him, he chooses without hesitation an ellipse. This ellipse, however, is widely different from the one which represents the shape of the inclined circle indicated by the laws of perspective, being much nearer to the circular form. The subject sees an inclined figure neither in its 'real' shape nor in the shape which is its perspective projection but as a compromise between these.

This result is equally at a variance with the view widely held by those who are not psychologists that the perceived characters of an object are those of its projection on the retina. This view is particularly to be found amongst writers on perspective. Thus the writer on perspective in the 14th edition of the *Encyclopaedia Britannica*(2) sums up the laws of perspective in a series of 'axioms' which are not given as axioms about the plane projection of solid figures but of how we perceive them. Thus: "Axiom 1. Parallel lines appear to approach one another as

[1] H. J. Watt, for example, says: "Generally a plate looks what it 'really' is, circular, not elliptical as the retinal image of it really is(1)."

they vanish, and to meet at an infinite distance from the observer in an imaginary point called the vanishing point of the system. Axiom 2. Parallel planes appear to approach one another as they recede from the eye, ..."

What this writer regards as axiomatic is that the characters of perception are identical with those of peripheral stimulation. While receding parallel lines do appear to converge, the proposition that they converge in appearance in the same way as they do in the projection on the retina or in a photograph (*i.e.* to the perspective vanishing point) is not only not axiomatic but experiment shows that it is not true.

The first experiments on apparent shapes were done with a subject looking from a controlled height at a circular or square disc lying on a table at a measured distance from the vertical line through the subject's eyes (Fig. 1). The purpose was to discover the apparent shape of the

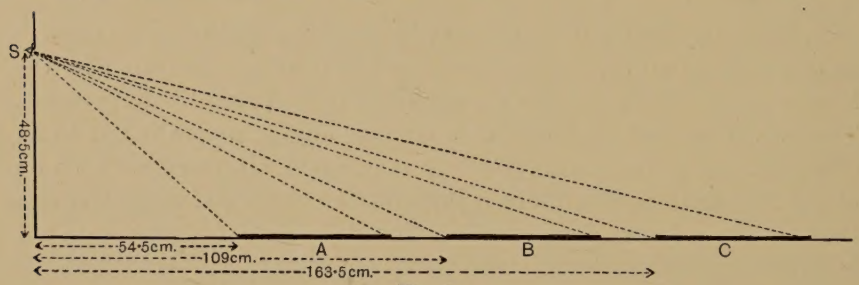

Fig. 1.

object from this point of view and to compare this with the shape of the perspective projection of the object on to a plane at right angles to the line of sight (*i.e.* with the shape which it would have in a photograph or in a drawing made in accordance with the laws of perspective). For the sake of brevity, I shall refer to this as the 'perspective shape' or 'stimulus shape.' The shape reported by the subject as seen by him may be called the 'apparent shape' or 'phenomenal shape.' This was at first measured by asking him to draw the disc as it appeared to him from that point of view. Later, it was found better to make the subject match the apparent shape of the circle with one of a series of ellipses cut out of cardboard with different ratios of short to long axis[1]. This ratio differed by 0·05 in successive ellipses. These ellipses were presented successively

[1] The perspective shape is not, of course, exactly an ellipse, but a figure resembling an ellipse with one of the short semi-axes longer than the other. The difference, however, is small and, since the judgments of the subject were only with respect to the relative lengths of the axes, it is of no importance for the purpose of the experiment.

to the subject (using the method of complete ascent and descent) and he was asked to judge whether the presented ellipse was 'fatter,' 'thinner' or 'the same as' the apparent shape of the circular disc. Preliminary practice was given, and the usual precautions of psychophysical experimentation were taken. These two methods I shall refer to as the 'drawing method' and the 'matching method,' respectively. Since it was no part of the aim of the drawing method to test the subject's drawing ability, he was allowed to alter his drawings as he pleased or to start them again until he had produced one which he was satisfied represented the shape as he saw it. In all experiments, except when otherwise stated, observation was with both eyes fully open and focussed on the object. The real shape of the disc used was therefore known by the subject.

The objects used were a white cardboard circle of 39·75 cm. diameter and a square of diagonal 38·0 cm. The object used lay on a dark table and was observed by the subject with his eyes 48·5 cm. above the end of the table (Fig. 1). The square was always placed with one of its diagonals in line with the subject. Three positions were experimented with: *A*, in which the nearest point of the object was 54·5 cm. from the point below the subject's eyes; *B*, in which it was 109 cm.; and *C*, in which it was 163·5 cm. from the same point.

Tables I and II show respectively the results for the subject S. drawing the circle and the square respectively. Table I shows all results (except of preliminary practice); Table II shows mean results of nine experiments at each position of the object. The figures given in the first and second columns are the ratios of short to long axis in the reproduced

Table I. *Drawings of circle by subject S.*

	Circle at *A*			Circle at *B*			Circle at *C*		
	Reproduced ratio	Perspective ratio	Index of phenomenal regression	Reproduced ratio	Perspective ratio	Index of phenomenal regression	Reproduced ratio	Perspective ratio	Index of phenomenal regression
(1)	·755	·56	·50	·575	·36	·46	·455	·255	·42
(2)	·76	·56	·53	·60	·36	·50	·445	·255	·41
(3)	·82	·56	·66	·56	·36	·43	·51	·255	·49
Mean	·78	·56	·57	·58	·36	·465	·47	·255	·445

Table II. *Mean of nine drawings of square by subject S.*

Square at *A*			Square at *B*			Square at *C*		
Reproduced ratio	Perspective ratio	Index of phenomenal regression	Reproduced ratio	Perspective ratio	Index of phenomenal regression	Reproduced ratio	Perspective ratio	Index of phenomenal regression
·86± ·03	·565	·74	·73± ·04	·36	·69	·58± ·035	·255	·60

figure and in the perspective shape respectively; those in the third columns are measures of the degree to which the 'real' determines the 'seen' shape, calculated by a formula explained below.

The same results are shown diagrammatically in Fig. 2. In each diagram the inner blackened figure shows the perspective shape of the object, the outer broken line marks its true physical shape, while the continuous line shows the mean reproduced figure (*i.e.* its phenomenal shape). In all cases it will be seen that the reproduced figure lies between

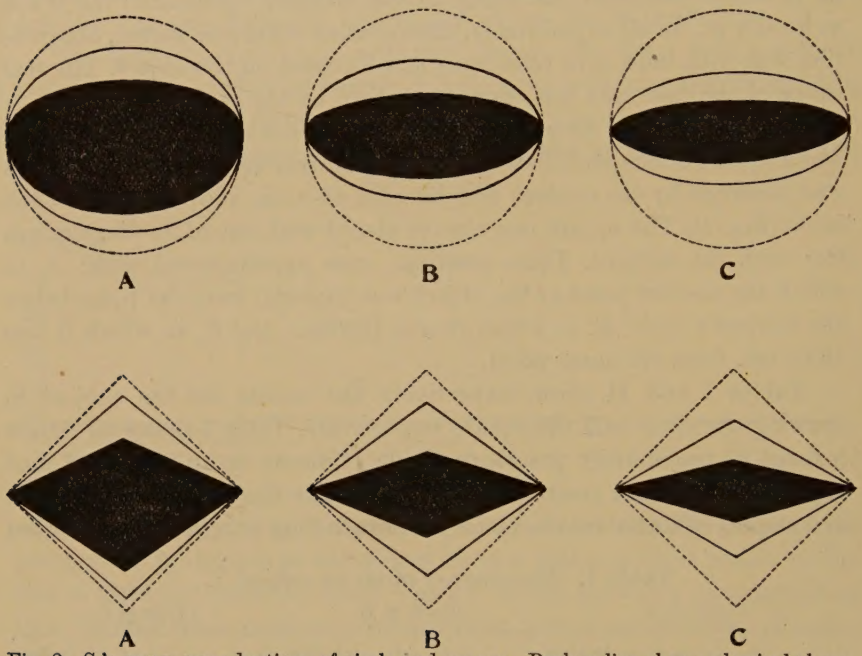

Fig. 2. S.'s mean reproductions of circle and square. Broken line shows physical shape of object. Black figure shows its perspective shape. Continuous line shows mean shape of reproduction.

these two extremes, and that it sometimes lies nearer to the physical shape than to the perspective shape. It is as if the known physical shape of the object distorted towards itself the seen shape from that which we should expect to result from the sensory cue of the image on the retina.

The first doubt that occurs to one's mind in attempting to explain these results is whether it is not possible that in all reproductions of ellipses or of trapezia there is a tendency to revert to the circular or square form. If this were the case, the assimilation to these shapes here found would not be the result of the stimulus being a projection of a

physically circular or square object but would be characteristic of any reproduction of an ellipse or a trapezium. This, however, was proved not to be the case by a subsidiary experiment.

The subject was given actually elliptical discs to reproduce from normal (*i.e.* not inclined) observation. The three ellipses given to the subject to copy had ratios of short to long semi-axis of 0·7, 0·45, and 0·25. The means of three reproductions by drawing of these were respectively 0·69, 0·45, and 0·245. Thus there is no tendency to revert to the circle in copying an ellipse[1]. The tendency observed when drawing an inclined circle or square must, therefore, be due to the effect of the actual shape of the physical object.

This experiment also eliminates the possibility that any appreciable part of the observed effect is due to irradiation of the white surface over the darker background. Any such action would take place equally whether the stimulating object were an ellipse viewed normally or an inclined circle giving the same retinal image.

In this failure of seen shapes to obey the laws of perspective, we are reminded of analogous phenomena in perception. Hering (3) showed that a white disc in shadow may appear brighter than a strongly illuminated grey disc even though the degree of shadowing is so great that the white is actually reflecting less light to the eye than is the grey disc. Apparent brightness is thus determined partly by the 'real' brightness or reflectivity of the object seen and not solely by the intensity of the retinal image. Similarly, if two objects of the same shape but different size are placed at such distances from the eyes that their apparent sizes are equal, it is found that their relative distances are such that the retinal image of the 'really' larger object is considerably smaller than that of the other. Apparent size is a function of 'real' size as well as of size of retinal image. In later sections, it will be shown that in these two cases also the same law of compromise holds. Under ordinary conditions of binocular vision, the actually experienced character of the object (or the 'phenomenal character') is a compromise between the 'real' character of the object

[1] There is, however, distortion in copying ellipses with larger ratio of short to long semi-axis. An ellipse with short axis vertical and of ratio 0·95 was copied as an ellipse of ratio 0·93, while a true circle normally observed was copied as an ellipse of ratio 0·975 (short axis vertical). The distortion is small and in the opposite direction to that due to phenomenal regression. It is probably due to the 'horizontal-vertical illusion.' It was one reason for later abandoning the drawing method in these experiments. The other reason was the possibility that training in drawing may tend to condition the drawing response to the stimulus shape even though the subject's perception of the object would be intermediate between the stimulus shape and the 'real' shape if this perception were tested by some form of response in which the subject had received no previous training.

and the character given by peripheral stimulation, whether this character is shape, relative size, or relative brightness. In all of these cases, the phenomenal character shows a tendency away from the stimulus character towards the 'real' character of the object. As a general name for this tendency, in whatever kind of perceptual character it is found, we may use the term *phenomenal regression to the 'real' object* or, more shortly, *phenomenal regression.*

It is also convenient to have a numerical measure, applicable to any perceptual character, of the degree to which this regression takes place. Let us use the symbol S for a stimulus character (*e.g.* the ratio of short to long axis in the perspective shape of square or circle in the above experiments), the symbol P for the corresponding phenomenal character (the corresponding ratio in the figure matched or drawn by the subject), and R for the corresponding 'real' or physical character of the object (the ratio in the actual object—unity in the square or circle). An obvious measure of the degree of regression of the phenomenal character away from the stimulus character towards the 'real' character of the object is the fraction of the distance separating the real from the perspective character over which the phenomenal character has regressed: *i.e.* the fraction $(P - S)/(R - S)$. This proves, however, not to be a satisfactory measure, since it leads to certain anomalies (particularly when used for the brightness and size regressions). A formula, only a little more complicated, which is free from these difficulties is

$$(\log P - \log S)/(\log R - \log S).$$

This is the measure which I have used throughout these experiments and have called the *index of phenomenal regression.* Its value is zero if there is no phenomenal regression, that is, if the phenomenal character is identical with the stimulus character; while it is unity if regression is complete, that is, if the phenomenal character coincides with the 'real' character of the object.

Determinations of the phenomenal shape of the inclined circle were also made with other subjects by the matching method. Mean results by this method with S. and other subjects are shown in Table III. It will be noticed that there are considerable individual differences in the amount of phenomenal regression.

The results (Table III) show that the index of phenomenal regression varies for different inclinations of the object to the line of vision. It seems also to vary somewhat with the size of the object, its distance and its shape (whether square or circular). The three latter

sources of variation were not investigated. An experiment was performed, however, to discover how the amount of phenomenal regression varied with different angles of inclination of the object. For this purpose, a white circular disc of 29·7 cm. diameter was mounted on a turntable with its axis of rotation horizontal and at right angles to the subject's line of vision. The disc was mounted with a diameter in line with the axis of rotation of the apparatus, so that turning the apparatus presented the disc to the subject at varying angles of inclination. The centre of the disc was 142·5 cm. from the subject's eyes. Eight observations in each position were made with the subject S. by the matching method at

Table III. *Mean matchings of phenomenal shape of inclined circle by four subjects.*

	Circle at *A*			Circle at *B*			Circle at *C*		
Subject	Matched ratio	Perspective ratio	Index of phenomenal regression	Matched ratio	Perspective ratio	Index of phenomenal regression	Matched ratio	Perspective ratio	Index of phenomenal regression
S.	> ·7*	·56	> ·39	·495	·36	·31	·46	·255	·43
X.	·74	·56	·48	·59	·36	·50	·43	·255	·38
B.	·785	·56	·58	·56	·36	·43	·435	·255	·39
D.	·84	·56	·70	·725	·36	·685	·645	·255	·68
M.	—	—	—	·575	·36	·46	—	—	—

* No standard ellipse of larger axis-ratio than this was available at the time of experimenting with S.

approximately the following angles of inclination: 7°, 10°, 20°, 30°, 45°, 65°, and 90°. The angles could not themselves be measured with sufficient accuracy for exact calculation of their perspective shape in each position, so this was done by placing a camera in the position of the eyes and measuring the ratios of the photographed ellipses with a travelling microscope. The results of this experiment are shown as a graph in Fig. 3.

In this figure, the broken line shows the variation of phenomenal shape with perspective shape. If the seen shape were identical with the perspective shape, all observed values would fall on a straight line passing through the origin and inclined at an angle of 45° to each axis. A continuous line has been drawn in this position. The amount of phenomenal regression is, therefore, shown by the height of the broken line above the continuous line. The diagram indicates that the amount of phenomenal regression diminishes to zero as the angle of inclination approaches 90° and (less certainly) as it approaches 0°. The indices of phenomenal regression are as follows: 7°, 0·28; 10°, 0·41; 20°, 0·33; 30°, 0·32; 45°, 0·16; and 65°, 0·1.

It has already been shown by the earlier experiment in reproduction of actually elliptical figures viewed normally that these results are not the consequence of a tendency to prefer the circular shape in perception and to assimilate other closed curves to the circular form in reproduction. This is shown more strikingly in the following experiment, in which the regression was from the circular shape and not towards it.

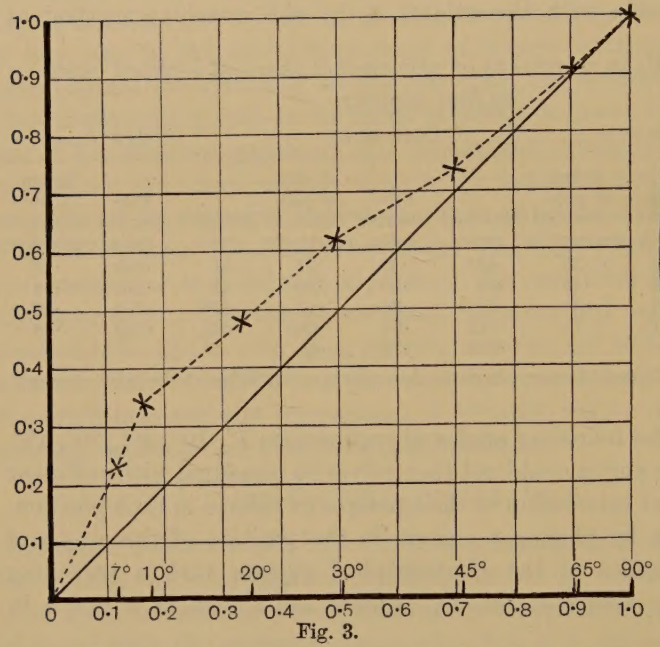

Fig. 3.

Fig. 4.

Fig. 3. Variation of phenomenal shape and of index of phenomenal regression for different inclinations of circular disc to line of vision. *Horizontal axis.* Ratio of axes in stimulus shape (corresponding angles of inclination shown above the axis). *Vertical axis.* Ratio of axes in phenomenal shape.

Fig. 4. S.'s mean matching of phenomenal shape of elongated ellipse so arranged that its perspective shape is approximately circular. Broken line shows physical shape of object. Black figure shows its perspective shape. Continuous line shows mean phenomenal shape.

Instead of using a circle as stimulus object an ellipse was used with long axis pointing away from the subject and the ratio of the axes was so chosen that the perspective figure would have equal axes (*i.e.* would be as nearly a circle as it is possible to get with a perspective projection of an ellipse). The actual shape of ellipse necessary in the position used

was found to have a ratio of long to short axis of 3·95. The result of this experiment is shown in Fig. 4. If the tendency were simply to assimilate the phenomenal figure to a circle, then no distortion should take place. In fact, there is still distortion and its direction is away from the circle and towards the physical shape of the object perceived—the ratio of the phenomenal shape was 2·65. It is the physical shape of the object, therefore, and not a preference for the circular form that determines the change in the phenomenal shape.

It is also possible, of course, to arrange an elliptical object so that the phenomenal shape is itself circular. The condition for this is that a sufficiently elongated ellipse with its long axis pointing from the subject shall cast on the retina an image of an ellipse with long axis horizontal. This case is of no special theoretical interest[1].

The full series of experiments from which the example in Fig. 4 was drawn is shown in Table IV. The ratios in columns 2 and 3 are of vertical to horizontal axes, in column 1 of axis in line with subject to that at right angles to him. It will be seen that not only is there no tendency for phenomenal regression to diminish as the perspective shape approaches circularity, but even that under those conditions the index found was greater than with any other perspective shape.

Table IV. *Mean matchings of phenomenal shapes of various elliptical discs viewed obliquely.* (*Subject S.*)

Ratio in observed disc of axis in line with subject to that at right angles to him	Ratio of vertical to horizontal axis of perspective shape	Mean ratio of vertical to horizontal axis of phenomenal shape	Index of phenomenal regression
3·93	1·05	2·65	0·70
2·01	0·54	1·19	0·60
1·33	0·355	0·825	0·64
1	0·27	0·50	0·47
0·75	0·20	0·40	0·52

II. HERING'S OBSERVATIONS ON *Gedächtniss-farben.*

In his *Grundzüge der Lehre vom Lichtsinn* (3), Hering records a series of observations on the perception of relative brightnesses and colours, of which the following is typical. If a piece of grey paper is placed near a

[1] It is, however, one of the most convenient methods of demonstrating the effect. If we discover the ellipse which, with its long axis pointing from the subject, appears to him at a given inclination approximately circular (*i.e.* with equal horizontal and vertical axes), we can demonstrate to the subject the fact that the perspective figure is really a flattened ellipse by allowing him to look at the object through a circular aperture held at right angles to the line of vision at such a distance from the eyes that the longest axis of the perspective ellipse just fits inside the circle.

window and compared with a piece of white paper so much farther away from the window that its luminosity is less than that of the grey paper, the grey paper is, nevertheless, seen as grey and the white as white—*i.e.* the less luminous surface of the white paper is seen as the brighter. If the two pieces of paper are now simultaneously examined with one eye looking through a blackened tube, the apparent brightness relationship is reversed, the white paper appearing to be of a darker shade of grey than the other. If they are again examined as part of the full field of vision of both eyes, the brightness relationship is seen as at the beginning—the less luminous white paper is seen as the brighter.

Similar effects were observed with tinted objects whose apparent hues were found to tend to remain constant in spite of changes in colour of the illuminating light. These shades or hues, which persisted in spite of changes in illumination, Hering called *Gedächtniss-farben* (usually translated as *memory colours*), and he picturesquely describes us as seeing certain classes of objects "through the spectacles of memory colours."

A mere repetition of the experiment on brightness described above might lead us to a conclusion which a more careful study shows to be wrong. The white paper appears brighter than the grey, although so much less illuminated that its luminosity is actually less. We might be led to suppose, therefore, that there is a law of absolute 'constancy of brightness' by which the paper of greater reflectivity[1] appears the brighter under any illumination however much reduced.

Further experiment, however, shows that the matter is less simple. If the white paper is further shadowed, a point is reached at which it appears of the same brightness as the grey paper, and if the reduction of its illumination is carried beyond this point, the white paper appears the darker. We are again dealing with a compromise effect, the apparent brightness is neither determined solely by the 'real' character of the paper's reflectivity nor solely by the stimulus character of luminosity, but is a compromise between them.

The fact of this compromise may be made clearer by a quantitative example. I took two squares of paper (20 cm. × 20 cm.) vertically mounted on cardboard. One was white, *A* (slightly brighter than the no. 1 of Zimmermann's scale); the other was a light grey, *B* (between

[1] I am using this word 'reflectivity' for the position of the paper itself on the white-black scale in order to avoid the ambiguity attached to the term 'physical brightness.' Psychologists commonly refer to this quality as the 'brightness' of the paper. Physicists, however, generally use 'brightness' in the same sense as 'luminosity' for the intensity of reflected light under given conditions of illumination. For this I shall use the word 'luminosity,' reserving 'brightness' for the phenomenal character.

Zimmermann's 3 and 4). Their relative reflectivities were first determined by rotating a sector of the white before a blackened chamber and matching with the grey. The amount of the white required for a match was found to be 135·5°, so the reflectivity of A was 2·65 times that of B. The two papers were normally illuminated in a darkened room by a 4-volt (1·2 watt) electric bulb in a blackened case with a filament of size negligible compared with the distance of the papers. The intensity of illumination of the papers was, therefore, inversely proportional to the square of their distances from the lamp.

The subject S. looked at the two papers with both eyes open and his head in such a position that the papers were not immediately adjacent but separated by a short space of black background. B was 100 cm. from the lamp and the mean distance at which A was found to be of equal phenomenal brightness was 184 cm. At a greater distance, A appeared the darker. At this distance of 184 cm., the calculated luminosity of A was 0·785 that of B. In other words, a paper about $2\frac{1}{2}$ times as reflective as another appeared equally bright to binocular observation when its actual luminosity was about three-quarters that of the other.

The white paper was next adjusted to phenomenal equality with B, but observed under different conditions. The subject looked with one eye through a blackened tube which cut out all surrounding objects, and with his head in such a position that he saw through the tube the surfaces of the two papers adjacent to one another. The mean distance of A at which the papers appeared equally bright was now 162·5 cm. Calculation shows that the ratio of the luminosity of A to B was now 1·0035 : 1; that is, the luminosity of the two papers was, as nearly as possible, equal.

A similar experiment was performed with the same subject on another day with a paper C of a much darker grey (no. 41 of Zimmermann's brightness scale of 50 shades). This was adjusted by the same light to phenomenal equality with each of the other two papers at 507·5 cm. from the same lamp as was used in the earlier experiment. In this case, the reflectivity of C was not determined independently but was assumed to be given by adjusting to apparently equal brightness with monocular observation through a blackened tube. In this experiment the effect of the inclination of C to the source of light was not negligible, so its luminosity was calculated from the formula:

$$\text{luminosity} \propto \text{reflectivity} \times \frac{\cos\theta}{d^2}.$$

The results of this experiment and of the earlier one are given in Table V.

Table V. *Phenomenal regression of papers of differing reflectivity so illuminated that they appear of equal brightness.*

Papers used	R_1/R_2	P_1/P_2	S_1/S_2	Index of regression
A and *B*	2·65	1	·785	·19
B and *C*	4·95	1	·298	·43
A and *C*	13·1	1	·175	·405

We are here dealing with an effect analogous to that of the phenomenal regression of perceived shapes. The relative reflectivity of the papers is the physical property of the papers themselves corresponding to the 'real' shapes of perceived figures. Their relative luminosity (which is equal to the relative luminosity of the retinal images) is the character of peripheral stimulation corresponding to the perspective shapes of the figures. Their relative phenomenal brightness (adjusted in this experiment to unity) corresponds to the phenomenal figure. The effect observed is that the relative phenomenal brightness is not determined solely by the relative stimulus brightness (*i.e.* relative luminosity) of the papers but also by their relative 'real' brightness (*i.e.* relative reflectivity). The relative phenomenal brightness is, in fact, a compromise between these two. As was observed with the perception of shapes, the greater the difference is between the reflectivities of the papers used, the greater must be the difference in their relative luminosities if they are to appear equally bright. If we indicate the relative reflectivities, luminosities, and phenomenal brightnesses by the symbols R, S and P respectively, the index of regression is given by the formula

$$(\log P - \log S)/(\log R - \log S),$$

or (since P is unity) by $(-\log S)/(\log R - \log S)$.

In a beautiful and ingenious series of experiments Köhler (4) has shown that both chimpanzees and hens show the tendency to constancy of brightness. He demonstrated that when these animals are trained to react to the brighter of two papers equally illuminated, they continue to react to the more reflective paper even when its illumination is so much reduced that it has a lower luminosity. He did not, unfortunately, investigate the further problem of the maximum degree of lowered luminosity of the whiter paper at which it still called out the same response and beyond which it was reacted to as the darker paper. It is not, therefore, possible from his results to calculate an exact value for the phenomenal regression but only a value which it must have exceeded.

With chimpanzees, the greatest disparity of illuminations was when the whiter paper (no. 3 of the Zimmermann scale) had a luminosity

0·0795 that of the darker (no. 41). If we take their relative reflectivities as about 5, this gives an index of regression greater than 0·61. There is some indication that the stimulus difference is here reaching the value at which reversal of the reaction would take place, since the ape makes two wrong reactions out of ten (although he makes none at all when the stimulus difference is less). It is, therefore, probable that the index of regression does not much exceed this value.

With hens there was no sign of an approach to the point of reversal when the relative stimulus values were 0·0807 : 1 with the papers 3 and 30 of Zimmermann's scale. I do not know the relative reflectivities of these papers, but since their difference is less than that between the papers used for the apes, the index of regression for the hens must be considerably greater than 0·61.

Katz(5) has used human subjects in an experiment in which rotating colour wheels with black and white sectors were adjusted to phenomenal equality when one was in shadow, and obtained results which give indices of phenomenal regression ranging in one experiment from 0·33 to 0·69.

Obviously, there are insufficient data for determining whether phenomenal regression is greater for chimpanzees and hens than for human beings, particularly since the experiments were not carried out under comparable conditions. The precise determination of the answer to this question would be an interesting problem for an animal psychologist.

III. Experiments on apparent sizes.

Another example of the same character of perception is to be found in the dependence of the apparent size of a seen object not only on the size of its retinal image but also on the physical size of the object, so that of two things producing equal retinal images the one that is more distant and actually larger also appears the larger. In order to measure this effect, two white circular discs of different sizes were fixed upright on movable stands, which were adjusted to different distances from the subject along two graduated lines on a table so diverging that there was no overlap of the retinal images of the two discs. The arrangements for viewing the discs were similar to those shown in Fig. 1. The larger disc remained fixed, while the distance of the smaller disc was varied until the subject reported that the apparent sizes of the two discs were equal. The 'limiting method' of experimenting was used. The perspective sizes of the two discs were then calculated from their diameters and their distances from the subject's eyes.

When the phenomenal sizes were thus adjusted to equality, in all cases the physically larger disc was at such a distance that its perspective or stimulus size (*i.e.* the size of the image actually cast on the retina) was considerably smaller than that of the other. In other words, phenomenal size also is a compromise between stimulus size and the physical size of the object.

Fig. 5 represents the mean of two sets of experiments with the subject S. The discs were 39·7 and 29·7 cm. in diameter, and appeared equal when they were at 240·5 and 117·5 cm. from the eyes respectively. Thus the larger disc, which was only about four-thirds the diameter of the other and a little less than twice its area, had to be slightly more than

Fig. 5. Conditions for phenomenal equality of two circular discs of different physical size (Subject S.). Broken line shows relative physical size of larger disc. Black figure shows its relative stimulus size. Continuous line shows its relative phenomenal size.

twice its distance from the subject for the two discs to appear equal. At this distance the diameter of the retinal image of the larger disc would be about two-thirds and its area less than half that of the smaller. In the diagram, the dotted circle (R) has been made proportional to the ratio of the physical size of the large disc to that of the small one, the inner blackened circle (S) to the ratio of their stimulus sizes, and the continuous circle (P) is proportional to their relative phenomenal sizes (*i.e.* to unity). If there were no phenomenal regression, the circle P would coincide with S whatever might be the size of R.

The index of phenomenal regression is calculated exactly as for the experiments in phenomenal brightness. R is the relative size of the

actual objects, S their relative stimulus size, and P their relative phenomenal size (unity in these experiments)[1].

Table VI shows mean results of this experiment for four subjects. The first two rows were made with the same subject on different days. The subject D. showed so large an amount of phenomenal regression that, when discs of diameters 39·7 and 29·7 cm. were used, the smaller disc was too near the subject for convenient measurement. Discs were therefore used with a smaller difference of size between them. If there were no phenomenal regression, row 8 would be, in all cases, unity. Phenomenal regression is shown by the tendency of the phenomenal diameter ratio (*i.e.* 1) not to coincide with the value in row 8 but to be intermediate between that and the value in row 5.

Table VI. *Mean results of experiments on phenomenal size.*

1	Subject	S.	S.	M.	D.	D.	C.	C.
2	No. of observations from which mean is calculated	12	8	6	—	8	4	8
3	Diameter in cm. of large disc (D_1)	39·7	39·7	39·6	39·7	29·7	39·7	29·7
4	Diameter in cm. of small disc (D_2)	29·7	29·7	29·7	29·7	26·5	29·7	26·5
5	Relative diameters (D_1/D_2)	1·335	1·335	1·333	1·335	1·12	1·335	1·12
6	Distance in cm. of large disc (L_1)	240·5	240·5	230	240	240	240	240
7	Mean distance in cm. of small disc for phenomenal equality (L_2)	120	114	114	< 70	162	98	162
8	Relative perspective diameters (D_1L_2/D_2L_1)	·665	·635	·66	$< ·39$	·755	·545	·755
9	Index of phenomenal regression $\frac{\log(8) - \log(8)}{\log(5) - \log(8)}$	·585	·61	·59	$> ·76$	·715	·68	·715

Here also, it is to be noted that we are dealing not with an absolute constancy of phenomenal size but with a tendency to constancy. At no distance from the observer is it true to say that changing distance of the object makes no difference to phenomenal size. As the distance of an object changes, its phenomenal size changes, whether the object be far or near. It changes, however, less rapidly than does the size of the retinal image. The tendency to constancy is shown by the amount of change being a compromise between the changing size of the peripheral stimulus and the unchanging 'real' size of the object.

In order to demonstrate this more fully an experiment was devised in

[1] It is an advantage of the formula $(\log P - \log S)/(\log R - \log S)$ over the simpler $(P - S)/(R - S)$, that the index has the same value whether the characters used in its calculation are the linear dimensions of the objects, their areas or their volumes. A more important advantage is the fact that it does not matter which of the objects compared provides the numerator and which the denominator for R, P and S. This is not true of the simpler formula. A further objection against the simpler formula is that it leads to absurdly small values of the index if the difference between the real sizes of the objects compared is great.

which a light circle was thrown on to a screen 5 m. from the subject, by means of a diaphragm of adjustable size in a projection lantern. A circular white disc supported vertically, of diameter 13·15 cm., was presented to the subject at distances of 1·33, 2, 3, 4, 5, and 6·5 metres. For each of these positions, the subject was required to adjust the circle cast by the lantern until it appeared equal to the disc. Fig. 6 shows the mean of ten observations in each position of the disc.

If the phenomenal size of the disc obeyed the laws of perspective, its changes would be proportional to the changes in stimulus size. The

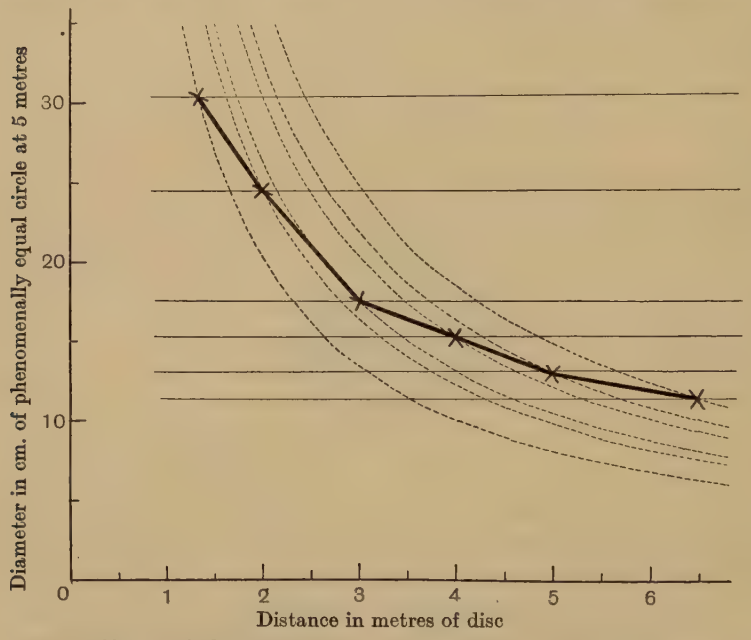

Fig. 6. Change of phenomenal size of circular disc with changing distance.

phenomenal length of diameter would, therefore, be proportional to the reciprocal of the distance of the disc from the eyes. All variations of apparent size with distance in Fig. 6 would lead to values lying on one of the series of curves $y = 1/x$, which is the curve of decrease of stimulus size with increasing distance. To show how far this is from being true of phenomenal size, I have drawn a curve $y = 1/x$ through each of the recorded values of the phenomenal size. The curve of change of phenomenal size cuts across these curves and follows a course intermediate between them and the curve of absolute constancy of shape (shown as a thin straight line parallel to the base through each recorded value). The

curve of decrease in phenomenal size is seen to fall all the way between these limits. Nowhere does the apparent size of the disc remain constant in spite of changing distance; nowhere does it change as rapidly as does the retinal image. The apparent change in apparent size is always a compromise between the change in stimulus size and the constancy of the 'real' size.

Köhler (4) has demonstrated that apes trained to react to the larger of two similar boxes continue to do so when its distance is so great that its perspective size is less than that of the other. Again, since he did not determine the distance at which the reaction was reversed (as from analogy with the above experiments we should expect it to be), no exact index of phenomenal regression can be determined. In Köhler's experiments R_1/R_2 (linear) was 4/3, while the greatest difference of stimulus sizes was when S_1/S_2 (linear) was 0·61. This shows that the index of regression was greater than 0·63. Since in my experiments, using discs of the same relative linear dimensions, most human subjects gave indices of regression greater than this, we cannot conclude whether or not the index of regression for chimpanzees is greater than that for human beings. A comparison of the results indicates that, at any rate, it is not much less.

IV. THE TENDENCY TO EQUALIZE VERTICAL SEMI-AXES IN PERCEPTION OF THE INCLINED SQUARE AND THE PHENOMENAL REGRESSION OF PARALLEL LINES.

In Section II, we considered only one respect in which the perception of the circle or square showed regression from the perspective to the physical figure—the ratio of the vertical to the horizontal axis. There is another character of the shape of the perspective figure which might also show phenomenal regression—the ratio of the upper to the lower semi-axis. In the perspective shape of an inclined square (Fig. 7), the

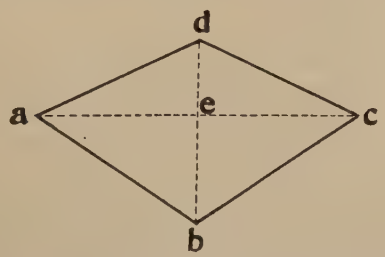

Fig. 7. Perspective shape of inclined square.

upper vertical semi-axis *de* is shorter than the lower one *eb*[1]; in the actual physical figure this ratio is unity. Phenomenal regression would, therefore, tend to make the ratio in the perceived or reproduced figure more close to unity than to its value in the perspective figure. Only a few observations were made on this matter. These indicated with certainty that this tendency to equalization of the vertical semi-axes is present in a large degree, although the number of observations was insufficient for exact measurement of its amount. Regression appeared to be more complete with respect to this character than in the character of equality of the ratios of the vertical and horizontal axes.

For this part of the investigation the square was necessarily used as object, since a reproduction of the circle gives no definite point from which the semi-axes can be measured. Also it was obviously more convenient to use the drawing rather than the matching method. The results of four experiments with the subject S. are shown in Table VII. It will be seen that for the nearest position of the object, this character showed the very large index of regression of 0·76, while for the other two positions it was not significantly different from unity, *i.e.* regression was apparently complete.

Table VII. *Tendency to equalize vertical semi-axes in drawing inclined square.*

Position of object	Ratio of semi-axes in perspective figure	Ratios of semi-axes in reproduced figure					Index of phenomenal regression
		1	2	3	4	Mean	
A	·69	0·96	0·95	0·88	0·875	0·915	0·76
B	·77	1·055	1·02	0·85	1·06	0·995	1·0
C	·82	0·97	1·02	1·115	0·92	1·005	1·0

It may be noted that this character is also an indication of the degree of convergence of opposite sides of the figure. The index of regression is a measure of the extent to which the converging lines of the perspective figure undergo regression in the phenomenal figure to the parallelism of the sides of the actual object.

There are other interesting consequences of this tendency of receding parallel lines to regress from the convergence of their stimulus character

[1] Their relationship is given exactly by the equation

$$de/eb = (D - R \cdot \cos\theta)/(D + R \cdot \cos\theta),$$

where D is the distance of the subject's eyes from the centre of the square, R is half the diagonal of the square, and θ is the angle between the plane of the square and the line of vision. Fig. 7 is actually the perspective figure for the square in position *A* of Fig. 1, where this ratio was 0·69.

to the parallelism of the 'real' object. In some cases, this tendency may result in a phenomenal divergence. A striking example of this is to be found by looking through a telescope or through binoculars at a wall with parallel top and bottom receding from the observer. Under these conditions, the top and bottom of the wall appear to diverge considerably as they go from the observer. The explanation of this appears to be that when looked at in the ordinary way the wall appears to converge, but much less so than does the retinal stimulus, regression having taken place to the 'real' character of parallelism. When looked at through a magnifying instrument, the wall appears nearer and the amount of phenomenal regression which takes place is not that proper to its actual distance but the greater amount proper to its apparent distance, so that the regression is beyond parallelism to divergence. Another way of stating this is that regression is taking place towards the non-existent object with divergent sides which, if it occupied the apparent position of the wall as seen through binoculars, would cast on the retina an image with the degree of convergence of that actually formed (this degree being, of course, considerably less than that from a parallel-sided object in the same position).

A more convenient method of showing the same phenomenon is by an isometric or other parallel-sided projection of a cube (Fig. 8). If we

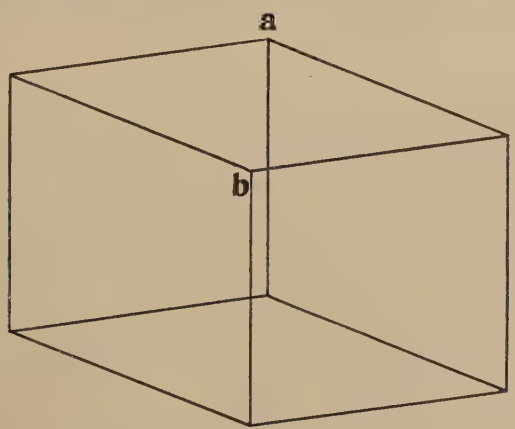

Fig. 8. Isometric projection of cube.

see this as a solid figure with the corner *a* towards us (a mode of perception favoured by the fixation of *a*), the two pairs of edges perpendicular to the vertical edge through *a* appear to diverge. A similar effect is seen in the other two pairs of edges if the phenomenal figure is reversed and *b* is seen towards us.

This may be explained by saying that this figure is a true perspective projection of an obliquely truncated pyramid with actually divergent sides[1]. This, however, is not a complete answer to the problem. If, as is commonly believed by writers on perception, the phenomenal object is entirely determined by the characters of the retinal projection, this figure should give a parallel-sided perception, whatever might be the shape of the object of which it is the projection. On the other hand, the phenomenal object is not a function only of the character of the object seen, since, if we look at an actual cube, the edges receding from us seem to converge somewhat. We can make a series of figures like Fig. 8 with the sides actually converging and select a member of the series in which the actual convergence exactly neutralizes the phenomenal divergence for one of the two ways of perceiving it. It is then seen as a parallel-sided figure. Similarly we can have a series of almost cubical solids with increasing actual divergence of sides and select one which appears parallel sided in perception. This would not be the one giving a parallel-sided retinal projection but one giving a retinal projection with sides still convergent but less so than that of the true cube. As with the perception of shapes, brightnesses and sizes, we are dealing with a compromise effect. The phenomenal character is a compromise between the character of the peripheral stimulus and that of the object (either 'real' or intuited).

V. Summary.

Experiments were performed on the shapes of objects viewed obliquely, the apparent brightnesses of differently illuminated surfaces of different reflectivity, the apparent sizes of objects at different distances, and the apparent convergence of parallel lines receding from the observer. In all of these cases it was found that what was seen was intermediate between what was given in peripheral stimulation and the 'real' character of the object. To this effect of the character of the 'real' object on the phenomenal character we may give the name 'phenomenal regression to the real object.' We may use as measure of this effect the index

$$(\log P - \log S)/(\log R - \log S),$$

[1] More precisely, of an infinite series of such solids of which the figure in which a and b are in the same plane (*i.e.* the projection itself) is a limiting member. There is the further psychological problem of why the phenomenal object should be a particular member of this series, and of why it should be so difficult to see the diagram as a plane figure with parallel sides since this is as much a member of the series of figures of which this could be a projection as any other. Possibly there is a tendency to approximate the phenomenon as nearly as possible to the cube.

in which P is a numerical measure of the phenomenal character, R of the 'real' character, and S of the stimulus character.

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IN WHAT SENSE CAN WE SPEAK OF PRIMARY COLOURS?

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I. Introduction.

At the present time it is mainly, though not exclusively, the physicist who speaks of 'primary colours.' The German equivalent 'Grundfarben' —in some respects a preferable term—is used, it is true, in the formulation and discussion of the Helmholtz theory of colour-vision by all and sundry, but in these discussions the physicist's point of view is still for the most part predominant. There is also, however, an entirely different point of view from which, and universe of discourse in which, both terms have been used—pictorial art. The colours designated 'primary' by the artist differ from those designated 'primary' by the physicist. The artist's primaries are red, yellow, and blue, the physicist's red, green, and blue. The difference is due to the difference between the two groups of phenomena, to which what is essentially the same logical principle of classification is applied. The artist finds that all the known bright colours can be derived from a mixture of red, yellow, and blue pigments. Vermilion and chrome-yellow give him orange; chrome-yellow and ultramarine green; vermilion and ultramarine purple, and so on. Arguing in the same way of spectral lights, the physicist claims that all the colours of the spectrum and of nature can be produced by the admixture in suitable proportions of red, green, and blue lights, but not by mixing red, yellow, and blue lights. He claims—and apparently with justice—that his point of view is more fundamental than that of the artist, that it is scientific and not merely empirical, that the artist's results in mixing pigments can be readily explained from this more

fundamental point of view, in fine that in a scientific sense his primaries must supersede the primaries of the artist. The question which the present paper aims at discussing is whether we really have a scientific and ultimate basis for the classification of the colours in the primaries of the physicist, and its general purpose is to show that formidable difficulties, and what appear to be striking inconsistencies with verifiable facts, are involved in the claims of the physicist. It seems indeed that the last word in this matter does not rest with the physicist at all, that as far as colour is concerned there is a still more fundamental point of view, and that the logical principle of classification which both artist and physicist have adopted is itself not above suspicion.

II. PHYSICAL VIEWS.

When Newton decomposed white light into its elementary components he sought to distinguish seven such components—red, orange, yellow, green, blue, indigo, and violet—and he represented them diagrammatically in the form of a circle, the completion of the circle being made by the addition of the purples between red and violet. The choice of the number seven was undoubtedly determined by the mystical value attaching to that number, and the same mysticism underlay the selection of indigo as an elementary colour, the motive in this case being to work out an analogy between the notes in the octave and the spectral colours—the music of the spheres. There is possibly also some unconscious mysticism underlying the later selection of the number three. In any case there is no reason at this point why the physicist as physicist should select any particular colours in the spectrum as elementary. Regarded physically, and with reference to the decomposition by a prism of white light into its components, the elementary spectral colours ought to be infinite in number.

The decomposition of white light into the spectral colours, together with the recombination of the spectral colours into white light, may be taken as the fundamental factual basis for a physical theory of colour, but, as Goethe pointed out long ago, not necessarily for a physiological or psychological theory. A second fundamental fact, and the fact upon which the mathematical treatment of the physical data—which we shall come to presently—is based, is the fact that by combining two spectral colours, not too far apart in the spectrum, we can, by suitably varying the proportions in which we combine them, produce all the intermediate colours. In this connection another very important point emerges. Physically regarded, the colour produced by mixing two homogeneous

colours—say red and yellow—is fundamentally different from the intermediate spectral colour which appears to the eye identical with it. When the mixture of lights is passed through a prism it is separated into the two bundles of rays with which we started, whereas in the case of the spectral colour, with which the mixture appears identical, no such result is produced by passing through a prism. Physically, therefore, the statement that we can produce a spectral colour by mixing two other spectral colours is contrary to fact. Psychologically and physiologically—though the latter is by no means certain—the mixture may be the same as the homogeneous colour, but physically the two are different. This very important fact seems to be entirely ignored in the mathematical treatment by the physicist of the phenomena of colour, which leads ultimately to the defining of the three 'primary' colours.

This mathematical treatment begins with the "Centre of Gravity Law of Colour Mixing," which was originally formulated by Newton. If we take two points A and B, not too far apart in the spectrum, then all the colours obtained by mixing the colours at A and B may be represented by a straight line AB, and any colour C, obtained by mixing an amount m of the colour A with an amount n of the colour B, is represented by the point C, the position of which is determined by the relation $AC : CB = m : n$ (Fig. 1). If now we take three colours in the spectrum,

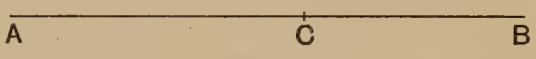

Fig. 1.

no one of which is producible by a mixture of the other two, then any colour p in the spectrum can be expressed by an equation of the form $p = xA + yB + zC$, where A, B, and C are the colours chosen in accordance with this principle. This is the second step in the mathematical treatment, and it permits us to represent all the colours of the spectrum geometrically by a triangle in place of Newton's circle.

But the three colours A, B, and C are still undefined. Theoretically indeed there may be an infinite number of groups of three colours which satisfy the conditions that no one is producible by a mixture of the other two, and that any colour p can be produced by mixing them in suitable proportions. For, if we take a new group of three colours, A', B', C', satisfying the first condition, it is clear that each of these can be expressed in terms of our original A, B, and C, and consequently p is expressible in terms of the new group by an equation of the form $p = x'A' + y'B' + z'C'$. Our primaries, therefore, or the vertices of our

colour triangle, have still to be defined. It has been suggested—by Abney, for example—that this difficulty is surmounted by adding to our original conditions the condition that no one of the three primaries is producible by a mixture of any two colours in the spectrum. But there is no colour in the spectrum that satisfies this condition in an absolute sense, *i.e.* if we neglect saturation, or, alternatively, if we take saturation into account, probably every colour in the spectrum satisfies it. The colours ordinarily recognized, however, as red, green, and blue—note the psychological criterion—cannot be produced in a high saturation by the mixture of two other colours. Accordingly these three colours are to be selected as the three primaries.

III. DIFFICULTIES IN THE PHYSICAL THEORIES.

New difficulties promptly make their appearance. Consider first the colour triangle as ordinarily represented (Fig. 2). If we place at the three

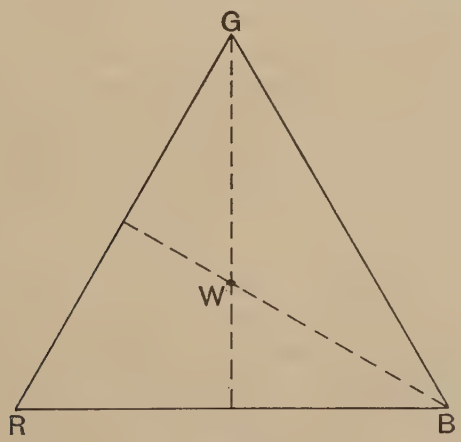

Fig. 2.

angles the red, green, and blue, which mixed in equal quantities give us white, we have an equilateral triangle, the sides of which represent respectively the colours of the spectrum from red to green, from green to blue, and from blue to red through violet and the non-spectral purples, while the centre of gravity of the figure—the centroid—represents white, and the different parts of the surface all the various hues of diminished saturation between white and the various spectral colours. It is very easy to show how inadequately the phenomena of colour are represented by this colour triangle. If the triangle is equilateral pre-

sumably the same number of distinguishable hues are to be found between *R* (red) and *G* (green), *G* (green) and *B* (blue), and *B* (blue) and *R* (red), which is contrary to fact. Moreover the colours nearest to *W* (white), and all equidistant from it, will be an orange-yellow, a blue-green, and a purple, which is also contrary to fact.

It is not necessary to go into the various attempts that have been made to meet these and similar difficulties. At the best they represent a compromise between the contentions of physiologist and psychologist, and the needs of the physicist, and the compromise can hardly satisfy either. It involves regarding the three primaries at the vertices of the triangle as mere points of reference, and at the same time colours "that never were on sea or land."

There is one other point that must be noticed before leaving the three primary colours of the physicist. Whether we take them as red, green, and blue, as is usually done, or as red, green, and violet, as many physicists prefer, it is obvious that scientific purposes will not be served unless we can define them more accurately than by the use of these popular and indefinite colour names. This more accurate definition has been attempted and with somewhat interesting results. König, for example, specifies as the three primaries a red complementary to $494\mu\mu$, but itself not in the spectrum, a green at $505\mu\mu$, and a blue at $470\mu\mu$. König's red, if we may so call it, is a mixture, or equivalent to a mixture, of spectral red with violet (or blue), which is of course inconsistent with the condition that no one of the three primaries can be produced by a mixture of spectral colours, and is not red psychologically. Reviewing the whole history of the three primary colours in physical theory, one cannot but feel that the colours originally chosen were not based on objective data, but, as already pointed out, were really those which appeared psychologically pure, or simple, but in order that objective criteria might be satisfied colours were finally chosen, which no longer satisfied the original psychological conditions. Tschermak, in a recent number of *Die Naturwissenschaften*[1], points out that the physicist's three primaries have invariably involved four physiological and psychological primaries, and this seems to be the case.

IV. Psychological criteria.

This situation naturally raises the question whether there are reliable psychological criteria, which will enable us to determine with some degree of accuracy the fundamental or primary colours, in the sense of

[1] Jahrgang xviii, Heft 26, 27 June, 1930.

those colours which are pure or simple, as distinct from those which look as if they were mixed colours. It ought to be possible to settle this question experimentally. In a preliminary series of experiments in the George Combe Laboratory at Edinburgh, two subjects with good colour-vision were tested with the Hue Discrimination Spectrometer. Each was asked to set the drum at a position at which the colour in the field seemed pure—red, yellow, green, or blue, as the case might be. The results obtained were as follows:

Red—average setting for both subjects 650$\mu\mu$, with a range of variation with the first subject of 30$\mu\mu$, and with the second subject of 15$\mu\mu$.

Yellow—average setting with the first subject 562$\mu\mu$, with a range of variation of 40$\mu\mu$, and with the second subject 569$\mu\mu$, with a range of 30$\mu\mu$.

Green—average setting with the first subject 505$\mu\mu$, with a range of 30$\mu\mu$, and with the second subject 512$\mu\mu$, with a range of 20$\mu\mu$.

Blue—average setting for both subjects 472$\mu\mu$, with a range of variation with the first subject of 4$\mu\mu$, and with the second subject of 1$\mu\mu$. (See also table below.)

The agreement for green and blue with König's figures is rather striking, but in all cases except blue the range of variation is much too great. Apparently, therefore, we shall have to admit the impossibility of arriving at a sufficiently stable determination of the psychologically pure or simple colours.

The position then would seem to be that, while the psychological criterion is to be preferred to the physical, and, that being so, the simple character of yellow is not for a moment in doubt, yet the variability and possibly more or less accidental character—conceivable dependence on the nature of the terrestrial environment—of the simple or primary colours determined on a psychological basis suggest the need of a more stable and objective criterion, if we are to recognize primary colours at all. Is there any characteristic of our organ of vision which can give us a basis for the classification of colours into primary and secondary, or simple and compound? When we ask this question the phenomena of perimetry at once suggest themselves as phenomena in which we may find an answer.

V. Peripheral colour vision and primary colours.

The exploration of the peripheral retina had its beginnings in the work of Thomas Young, published as early as 1801. Young, however,

was interested merely in the problem of the sensitivity of the marginal retina to light, and does not seem to have hit upon the colour phenomena. The different effects of different colour stimuli on the marginal retina were first investigated by Purkinje some twenty years later. Purkinje discovered that all light stimuli were colourless in the peripheral zone, that some stimuli showed change of colour between this zone and the position at which the true colours appeared, and that other stimuli did not show any colour change. Purkinje in fact discovered all the main phenomena, but he confessed himself unable to suggest an explanation. In 1840 Szokalsky, from results similar to those of Purkinje, drew the inference that the retina is composed of three concentric zones, the outermost zone being sensitive to black and white, the intermediate zone to black, white, blue, and yellow, and the innermost zone to those with red. Subsequent investigators added little of significance from our present point of view, though it is interesting to find Raehlmann and also Krükow in 1873 becoming aware of the difficulty of reconciling with the Young-Helmholtz theory the fact that yellow is sensed in parts of the retina where there is no sensing of red or green.

During the last two decades of the nineteenth century a series of investigations by Bull, Hess, and Hegg brought to light facts which are crucial for our present problem. The main object of these investigations was to delimit more clearly the colour zones in the retina, but in all cases one of the preliminary problems was to determine the stable colours ('unveränderlich' was the term used by Hess). In all cases such colours were found, and there were four. Hess gave as the wave-lengths of these stable colours: yellow 574·5$\mu\mu$, green 495$\mu\mu$, blue 471$\mu\mu$, the stable red being outside the spectrum and identical with König's red. In all these investigations coloured papers were used. In 1905, however, Baird published the results of experiments with coloured lights, which definitely confirmed the conclusions of Bull, Hess, and Hegg. Spectrometric analysis of the lights gave as the stable colours: yellow from 551$\mu\mu$ to 587$\mu\mu$, green from 483$\mu\mu$ to 500$\mu\mu$, blue from 448$\mu\mu$ to 474$\mu\mu$, red being again König's red beyond the visible spectrum. These results were confirmed by Dreher in 1911.

It seems, therefore, that four physiologically stable colours can be determined by perimetry. Colour changes with variation in intensity of illumination and with fatigue yield results in close agreement. The following table gives a selection of the results obtained by different investigators, so that agreements and differences may be seen at a glance:

	Red	Yellow	Green	Blue
König	Compl. to 494	—	505	470
Exner	,,	—	508	475
Goldmann	,,	568	504·5	468
Schubert	,,	575	503	468
Hess	,,	574·5	495	471
Baird	,,	569	491·5	461 (average)
Edin. 1st subj.	650	562	505	472
Edin. 2nd subj.	650	569	512	472

The physiologically stable blue and yellow agree fairly closely with the psychologically pure blue and yellow. On the other hand, the physiologically stable red and green do not look psychologically pure colours—both look bluish. Moreover, the physiologically stable red coincides with König's red, while the physiologically stable green is its complementary, and the psychologically pure green coincides with König's green. It is possible that the discrepancy between physiological and psychological red and green may be due to macular pigmentation, and to the fact that our standard light—that is, normal daylight—is yellowish. However that may be the discrepancy remains.

VI. Conclusion.

How are we then to answer the question with which we started? It is clear that the final decision does not rest with the physicist. Colour phenomena are physiological and psychological phenomena—in the last resort psychological. Psychologically there seem to be four simple or pure colours, and no more than four. Physiologically also there seem to be four stable colours and no more than four. But in respect of two out of the four psychologist and physiologist are not in agreement. Under the circumstances it appears to be the best plan to abandon the use of the word 'primary' altogether, except possibly in the artistic sense, and to speak of 'physiologically stable' and 'psychologically simple' colours, recognizing four in both cases. The number three has no rational basis, but probably, as we have seen, a mystical. In any intelligible sense of 'primary,' if we are to use the term at all, the physiologist has good grounds for maintaining that there are only two 'primary' colours—blue and yellow. Psychologically there are undoubtedly four.

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CHARACTERISTICS OF PROOF-READING.

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I. Introduction.

It has been abundantly proved in the past that when we read we do not perceive separately and individually every letter of every word of the text. Our speed of reading is such that we cannot gain more than a vague general impression of the word, and possibly of some of its more prominent letters. The proof-reader, on the other hand, must perceive every letter of every word with sufficient clearness to detect even minute errors and misprints. What is it, then, which differentiates the reading of the proof-reader from normal reading? And are any special abilities or characteristics necessary for the efficient proof-reader? It seems probable that the proof-reader's perception of the words and letters is of the subliminal type which does not reach consciousness as long as it does not vary from the normal and habitual, but only when it is of an unusual nature. Thus it would be similar to the awareness of the cessation of a long-continued noise which had previously ceased to reach consciousness. But the normal reader is not incapable of this type of perception. Anyone who has attempted to read proofs will have been conscious of adopting an attitude of alertness and special receptiveness to any unusual feature of the printed material, that is to say, to any misprints. It seems possible, then, that the proof-reader may be able habitually to adopt a perfected form of this attitude when reading proofs. But we do not know what if any innate mental or physiological capacities are necessary, or at least favourable, to its inception. Nor can we assume that the proof-reader does not possess some specialized faculty or mode of procedure which enables him to detect misprints with an accuracy impossible to the ordinary reader.

Crossland(1) concluded from his work on proof-reading that some form of innate ability did exist. He compared the rate of reading of

proof-readers and their ability to detect misprints with those of readers untrained in proof-reading. He found that the trained readers as a group read more slowly than the untrained, and deduced that training had probably lengthened the fixation pauses during reading. But although on the average the trained proof-readers showed more accuracy in detecting misprints than the other readers, several of the latter were quite as accurate as the former, and some were more accurate. Thus the longer fixation pauses and slower reading times did not necessarily lead to more accurate perception. Also, when the frequency of the misprints in the proof was varied, on the whole the accuracy of the trained proof-readers varied less than that of the untrained; but it was found that, taking all the readers together, the most accurate ones were the least variable, rather than the trained ones. Thus Crossland concluded that some underlying ability for keen perceptual discrimination determined both the accuracy in detecting misprints, and the variability of that accuracy with the frequency of errors. Clearly some trait or mental 'set' enabled the accurate reader to detect errors with skill; but it is not easy to determine the nature of this trait. It appeared that the aim or object of the reader was a factor of some importance, since accuracy was much greater when special instructions for attention to misprints were given. Again, absorption in the meaning of the content seemed to decrease accuracy considerably. Since this occurred both with the trained and the untrained readers, Crossland considered that ability to ignore this meaning was not acquired with practice, and was probably innate when it did exist. It was found by Downey (2) that when sentences containing misprints were exposed tachistoscopically, the most intelligent subjects at first ignored the misprints, and read entirely for the meaning of the sentences, but when they realized the presence of the misprints, they quickly corrected themselves, and afterwards detected more misprints than the less intelligent subjects. From this Crossland deduced that general intelligence is correlated with proof-reading ability, and that the common factor may be innate ability for accurate perceptual discrimination, since this plays a considerable part in general intelligence as usually estimated. But the work of Whipple (6) and Gates (3) has shown that there is no such thing as general perceptual ability; the ability varies according to the nature of the stimulus object. And we may infer that the detection of misprints in ordinary proofs does not involve the same attitude or the same ability as does their tachistoscopic perception. The part played by the assimilation of the meaning and the general contextual setting is quite different in the two cases. Thus we may be led

to suppose that proof-reading ability may be to some extent connected with the temperamental basis which must underlie the attitudes or aims that enable the proof-reader to concentrate upon the perception of misprints.

But the results of Crossland may equally show that certain individuals are able when called upon to assume an attitude or mental 'set' such that the general meaning of the content is ignored, while the recognition of misprints occupies the centre of consciousness. And it seems quite possible that only long training will enable the reader to maintain this attitude indefinitely, without undue effort and strain, though the untrained reader can do so for a short period, perhaps with considerable effort. A study of the underlying mental processes concomitant with reading should throw some light upon these attitudes. It has been shown by the writer (4) that the variations in the eye movements and fixation pauses which occur in normal reading are in general closely related to the underlying mental processes. It was thus thought that some information as to the nature of the methods and abilities of professional proof-readers might be obtained by studying their eye movements in reading normal material and material containing misprints, and comparing them with the eye movements of other individuals who were not professional proof-readers. Moreover, Crossland's theory as to the unusual length of the fixation pauses of proof-readers could be tested, and also the importance to the accurate proof-reader of natural accuracy of eye movement and fixation.

II. Method of experiment.

The method by which the eye movements were recorded has already been described in detail in this *Journal* (5). In brief, a beam of light is directed on to the cornea of the right eye, reflected by it and focussed to form a magnified image upon a photographic film which travels vertically at a steady rate. Fixation pauses appear as a series of fine vertical black lines upon the film. A time record is also photographed upon the film, so that the duration as well as the number of the fixations can be measured[1]. Eight passages, each ten lines in length, of normal printed reading matter, dealing with various topics, were read by each subject. In addition were read eight passages, each twenty lines in length, of reading

[1] In practice it was found more convenient to arrive at the average duration of the fixations in the reading of each line of print by dividing the time taken to read the line by the number of fixations. The time taken by the inter-fixation movements was neglected, since it is small and regular.

matter of similar content and style; the second, third, sixth and seventh of these contained a number of misprints varying from nine to nineteen. The third passage is appended, as a specimen.

It was shown in a former series of experiments (4) that the number and duration of the fixation pauses made in normal reading appeared to be related to the accuracy of voluntary eye movement from point to point, and the steadiness of voluntary fixation upon a point. Thus in this series also records were taken of the movements of the eyes to and fro between points situated at visual angles of 5°, 10° and 20° to the left and right of the central point; and also of long fixations for periods of twenty seconds of points situated at these angular distances from the central point.

The subjects of these experiments were (1) four proof-readers, called *W*, *X*, *Y* and *Z*, from the Cambridge University Press, (2) five of the subjects, here called *A*, *B*, *C*, *D* and *E*, who had taken part in the previous series of experiments (4). Three of the proof-readers were 'press' readers; that is to say, they read the final proof before it was printed for publication, and read it more or less straight through without a word-by-word comparison with the original MS. They had been proof-readers for thirty, sixteen and ten years respectively. The fourth was a young man who had been proof-reading for two or three years only; he was a 'first-proof reader'—that is, he read the first rough proof from the printer, comparing it continually with the author's MS. Thus he had not only had much less experience than the other three, but was also less accustomed to rely upon his own acumen and judgment in detecting and correcting misprints. One qualification must be attached to the results obtained from the first three subjects; their eyesight was not normal, and two of them were so short-sighted that it was necessary to place the smaller printed material closer to their eyes than the normal reading distance.

Of the other five subjects, four were graduates and research workers in psychology. The fifth had a considerable knowledge of psychology, but less general education and less practice in reading than the others.

III. Results of the experiments.

In Table I are shown the average results for the nine subjects reading the twelve passages of normal reading material. It appears that the average time taken to read a line of print was slightly greater for the proof-readers as a group than for the other subjects; but the overlap was much too large to allow any conclusions to be based upon the difference. The standard deviations (S.D.), which measure the variation of the reading time from line to line, are, however, less for the proof-readers

than for any of the other subjects except *B*. Again, although on the whole the proof-readers made fewer fixation pauses per line than the other subjects, there was an overlap; but the standard deviation of number of pauses was less for the proof-readers than for any other subject except *B*. The average duration of the pauses was much the same for proof-readers and others, and so also were the standard deviations of duration[1]. But the regressions[2] were much fewer for the proof-readers, except Subject *Z*, than for the others, except Subject *B*. Now it is clear that frequent regressions prevent a methodical and regular type of reading. Hence we may deduce that the proof-readers, in reading normal material, are not much slower than the ordinary reader, but are on the whole considerably more regular and methodical, and less variable. This conclusion is immediately apparent on viewing the photographic records of the fixations; the almost machine-like regularity of the succession of pauses shown by the proof-readers may be contrasted with the irregularity of the other subjects. Subject *Z* provides an instructive exception. His tendency to regress was very similar to that of the

Table I.

	Reading time per line in sec.		No. of pauses per line		Duration of pauses in sec.		No. of regressions per line
Subject	Av.	S.D. from Av.	Av.	S.D. from Av.	Av.	S.D. from Av.	
A	2·61	0·528	10·4	2·05	0·255	0·0375	1·23
B	1·76	0·332	7·4	1·33	0·242	0·0330	0·16
C	2·68	0·677	9·1	2·03	0·302	0·0400	1·08
D	2·23	0·498	9·6	2·02	0·231	0·0261	1·27
E	2·41	0·500	11·2	2·20	0·218	0·0258	2·04
Av.	2·34	0·507	9·5	1·93	0·250	0·0325	1·16
W	2·93	0·343	8·8	1·09	0·342	0·0452	0·49
X	1·95	0·272	8·9	1·21	0·216	0·0239	0·31
Y	3·11	0·459	11·1	1·68	0·281	0·0293	0·48
Z	1·98	0·317	8·6	1·37	0·228	0·0307	1·39
Av.	2·49	0·348	9·4	1·34	0·267	0·0323	0·67

ordinary reader, and it is tempting to assume that it had not yet been eliminated by years of training.

But it was also pointed out that Subject *B* showed as little variation and as few regressions as did the proof-readers. This extreme regularity seems to have been due to the total lack of interest with which this subject read the material. In a former series of experiments (4) it was

[1] Had it been possible to measure the duration of each fixation pause individually, group differences between the standard deviations might have been more apparent.

[2] A regression occurs when the eye moves backwards from the word fixated to another before it in the line, or in the previous line.

found that when he was interested in the content of what he was reading, and was attending closely to the meaning of the content, his variability and number of regressions were considerably increased, although they were still less than those of Subjects *A*, *C*, *D* and *E*. Thus it appeared that a reader with a natural tendency to regularity of reading might be even more regular when scarcely paying any attention to the meaning of the content. This connection between lack of attention and regularity of the reading processes was manifested fairly generally by all the non-proof-readers. This supports Crossland's conclusion, that one qualification for a proof-reader is to pay little attention to the general meaning of the content. The introspections given by one or two of the proof-readers lead to the same conclusion. They stated that it was necessary to assimilate to some extent the meaning of the words and phrases, in order to correct grammatical errors and also contradictions and disparities of statement. But there was little or no comprehension of the ultimate direction of the argument, or the general conclusions of the reasoning, and, in all probability, none of the associated thought and imagery which constitute the ultimate significance of words and phrases. Similarly the proof-readers knew the correct spelling and even the correct use of technical terms and phrases without understanding in the least what they meant. Hence it seems probable that the most accurate correction of a proof is made by a reader who is not personally interested in the topic under discussion; or by one who by long practice has learnt to inhibit any such interest.

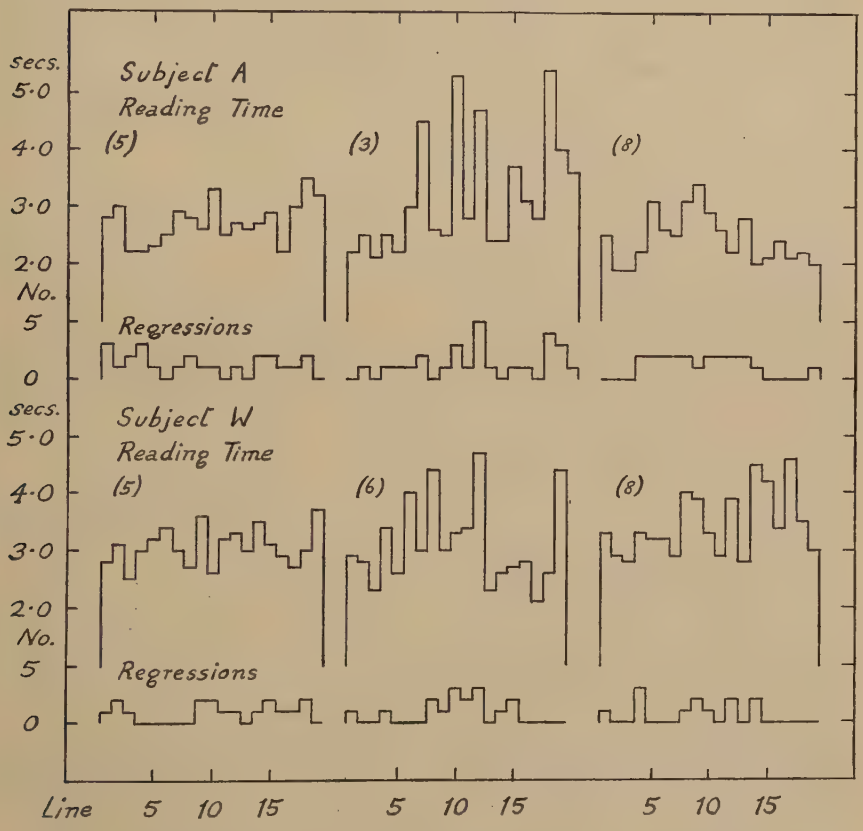
The same regularity of procedure appeared among the proof-readers when reading material containing errors. This procedure was not altogether comparable with actual proof-reading, because the readers were not able to stop and correct the errors. They often experienced a feeling of struggle and effort as a result of this, but stated that they were able to continue straightforward reading, without regression and without any difficulty in understanding the meaning of the content. The non-proof-readers, who experienced no habitual tendency to correct the misprints, were far more affected by them. After having noticed one or two obvious misprints, they usually adopted an alert, searching attitude—on the look-out for more misprints. This was frequently accompanied by pronounced affect—with feelings of irritation or amusement, or with heightened interest in the pursuit of 'spotting' the misprints. In consequence, interest in the meaning of the content usually diminished; and in one passage where there was a misprint in nearly every line, comprehension of the meaning was generally very vague, and sometimes com-

pletely absent. These attitudes were not infrequently carried over into the reading of the subsequent passages containing no misprints; but they usually disappeared during the latter half of the reading, once it had become clear that there were few if any misprints. Thus it appears that in the naïve subject anything unusual in the perceptual situation—the proof—arouses an attitude of scrutiny, often affectively toned, towards its minute details. But in the proof-reader this attitude has probably become habitual, and is only accompanied by affect when the normal processes are inhibited, that is to say, when the error is not immediately corrected.

These conclusions are supported by the variations in the eye movement processes which appeared in the reading of the material containing misprints. Thus in Table II it is shown that in general the difference of average reading time per line between the passages (1) and (5), containing no misprints, and passages (2), (3), (6) and (7), containing misprints, is considerably less for the proof-readers than for the other subjects; the same is true on the whole for the standard deviations of average reading time, and for the number of regressions. Thus the proof-readers were more regular in their normal reading than the other subjects, and much more regular in their reading of material containing misprints. This must not be attributed to the fact that the proof-readers were less quick and accurate in detecting the misprints than were the other subjects; the reverse was the case.

It also appeared that the slight increase of reading time and its standard deviation which did occur was more or less maintained by Subjects *W*, *X* and *Y* during the reading of passages (4) and (8), containing no misprints, showing that the attitude of alertness and scrutiny, once adopted in response to the occurrence of misprints, persisted to a greater degree with these subjects than with Subjects *A*, *B*, *D*, *E* or *Z*. This is also illustrated in the Figure, which shows the reading time for each line and the number of regressions for Subjects *A* and *W* reading passages (5), (3) and (8), and (5), (6) and (8) respectively. In the reading of passage (3) Subject *A*'s reading time and regressions suddenly increased and became irregular on his realizing the presence of the misprints; and decreased and became more regular in the latter half of passage (8), when he found that there were no misprints in this passage. Contrasted with this, the increase of reading time and regressions for Subject *W* reading passage (6) is slighter and more regular; but the irregularity persists all through the reading of passage (8). The number of regressions made by Subjects *W*, *X* and *Y* in reading passages (4) and (8) was not much greater

than that made in reading passages (1) and (5), which perhaps shows that the regressions in passages (2), (3), (6) and (7) resulted from the inability to correct the misprints in the accustomed manner. That is to say, they were an artifact of the experimental situation, and were not natural to the habitual procedure adopted in proof-reading; they did not occur in passages (4) and (8), where there were no misprints, because there was no habitual tendency to perpetuate them.



It was suggested by Crossland(1) that in proof-reading the duration of the pauses might be considerably lengthened, but this is not shown in these results. It is true that there was a slight increase of average duration for both groups of subjects between passages (1) and (5), and passages (2), (3), (6) and (7), and that the proof-readers remained at the higher level while reading passages (4) and (8), while the other subjects returned to the previous average duration. This is shown in Table III.

Table II.

Subject	Av. reading time per line in sec.					Standard deviation of reading time					Av. number of regressions per line				
	Av. for passages			Differences between passages		Av. for passages			Differences between passages		Av. for passages			Differences between passages	
	(1), (5)	(2), (3), (6), (7)	(4), (8)	[(2), (3), (6), (7)] − [(1), (5)]	[(2), (3), (6), (7)] − [(4), (8)]	(1), (5)	(2), (3), (6), (7)	(4), (8)	[(2), (3), (6), (7)] − [(1), (5)]	[(2), (3), (6), (7)] − [(4), (8)]	(1), (5)	(2), (3), (6), (7)	(4), (8)	[(2), (3), (6), (7)] − [(1), (5)]	[(2), (3), (6), (7)] − [(4), (8)]
A	2·67	3·06	2·94	0·39	0·12	0·460	0·836	0·553	0·376	0·283	1·08	1·59	1·60	0·51	− 0·01
B	1·78	2·19	1·83	0·41	0·36	0·234	0·744	0·406	0·510	0·338	0·13	0·62	0·26	0·49	0·36
C	2·58	3·34	3·28	0·76	0·06	0·619	0·837	0·944	0·218	− 0·107	0·95	1·57	1·47	0·62	0·10
D	2·26	3·05	2·66	0·79	0·39	0·604	1·205	0·617	0·601	0·588	1·47	2·31	1·76	0·84	0·55
E	2·56	3·03	2·78	0·47	0·25	0·499	0·793	0·514	0·294	0·279	2·35	3·32	2·99	0·97	0·33
			Av.	0·56	0·24			Av.	0·400	0·276			Av.	0·69	0·27
W	3·07	3·23	3·45	0·16	− 0·22	0·325	0·679	0·554	0·354	0·125	0·89	1·04	0·63	0·15	0·41
X	2·03	2·43	2·39	0·40	0·04	0·192	0·506	0·367	0·314	0·139	0·26	0·56	0·34	0·30	0·22
Y	2·89	3·46	3·64	0·57	− 0·18	0·431	0·558	0·539	0·127	0·019	0·40	0·55	0·44	0·15	0·11
Z	1·99	2·13	2·18	0·14	− 0·05	0·327	0·324	0·263	− 0·003	0·061	1·70	1·45	1·37	− 0·25	0·08
			Av.	0·32	− 0·10			Av.	0·198	0·086			Av.	0·09	0·21

Table III.

Subject	Av. duration of pauses in sec.					Av. number of pauses per line				
	Av. for passages			Differences between passages		Av. for passages			Differences between passages	
	(1), (5)	(2), (3), (6), (7)	(4), (8)	[(2), (3), (6), (7)] − [(1), (5)]	[(2), (3), (6), (7)] − [(4), (8)]	(1), (5)	(2), (3), (6), (7)	(4), (8)	[(2), (3), (6), (7)] − [(1), (5)]	[(2), (3), (6), (7)] − [(4), (8)]
A	0·243	0·260	0·251	0·017	0·009	11·1	11·8	11·7	0·7	0·1
B	0·225	0·246	0·224	0·021	0·022	7·9	9·4	8·3	1·5	1·1
C	0·310	0·325	0·293	0·015	0·032	9·4	10·5	11·3	1·1	− 0·8
D	0·229	0·235	0·239	0·006	− 0·004	9·8	12·8	11·3	3·0	1·5
E	0·216	0·217	0·217	0·001	0·000	11·9	14·2	13·1	2·3	1·1
				Av. 0·012	0·012				Av. 1·7	0·6
W	0·304	0·304	0·338	0·000	− 0·034	10·2	10·7	10·4	0·5	0·3
X	0·232	0·237	0·215	0·005	0·022	8·9	10·3	10·4	1·4	− 0·1
Y	0·276	0·293	0·277	0·017	0·016	10·5	11·9	13·1	1·4	− 1·2
Z	0·233	0·236	0·237	0·003	− 0·001	8·9	9·1	9·1	0·2	0
				Av. 0·006	0·001				Av. 0·9	− 0·3

The same applied, however, to the average number of fixation pauses per line. Thus it appears that the presence of misprints caused an increase both of number and duration of fixation pauses, which was slightly though not significantly greater among the non-proof-readers than among the proof-readers, but which persisted for the proof-readers during the subsequent reading of material without misprints. The misprints did not set up any differentiating effect between number and duration of fixation pauses, but merely produced a general increase of reading time per line. Nor had the fixation pauses of the proof-readers become permanently lengthened by long practice in proof-reading; the range of pause duration was very similar for all the subjects in reading all kinds of material.

Thus we may conclude that there is little obvious difference in objective procedure between the trained proof-reader and the ordinary practised reader. But clearly the proof-reader is more regular in reading normal material and still more in reading material containing misprints; his reading time and number of fixations are less variable, and he makes very many fewer regressions. He is able to deal with material riddled with misprints, even when these are unexpected, in a systematic and methodical fashion, without getting confused or allowing his eyes to wander wildly backwards and forwards.

If the only characteristic of the proof-reader's eye movements which differentiates them from those of the non-proof-reader is their much greater regularity, it is interesting to discover whether there are any characteristics of voluntary eye movement which differentiate the proof-reader. It was found that all readers tended to let their eyes fluctuate and wander away from the fixation point both during the long fixation periods of twenty seconds, and also during the short fixation periods between voluntary movements. It was difficult to measure the degree of fluctuation in these cases. But a rough estimate is provided by counting the number of fluctuations which could be detected. In Table IV these numbers are given for each subject as a percentage of the average for all subjects. It will be seen that on the whole the proof-readers made fewer fluctuations than the other subjects. The difference was more clearly marked in the short fixation periods than in the long ones; and in the latter, Subject *W*'s fixations were quite exceptionally fluctuating. This inability to fixate may have been due to ocular defect, since it was most pronounced. Table IV also shows that the proof-readers were among the more accurate in moving their eyes from one point to another—that is to say, they did not show so strong a tendency to stop

short of or go beyond the correct point, and then make corrective movements. If the subjects are ranked in order from the steadiest and most accurate to the least steady and most inaccurate in voluntary fixation and movements, it appears that the average ranks of the proof-readers were higher than the average ranks of all the other subjects except *C*

Table IV.

	No. of fluctuations as per cent. of av. no. of		No. of inaccurate fixations per ten movements	Av. inaccuracy of movement in minutes of angle	Rank of subject in: No. of fluctuations in		No. of inaccurate fixations	Av. inaccuracy of movement	Av. rank of subject
Subject	Long fixations	Short fixations			Long fixations	Short fixations			
A	125	138	8·2	53·3	7	9	7	6	7·3
B	91	110	8·1	55·7	5	6	4	8	5·8
C	58	94	6·9	40·7	3	5	1	3	3·0
D	107	136	8·8	54·6	6	8	8	7	7·3
E	163	125	8·9	55·7	8	7	9	8	8·0
Av.	108·8	120·6	8·2	52·0	5·8	7·0	5·8	6·4	6·3
W	198	78	7·5	32·1	9	3	3	1	4·0
X	25	85	8·1	47·1	1	4	4	4	3·3
Y	82	62	8·1	52·5	4	1	4	5	3·5
Z	47	67	7·0	38·6	2	2	2	2	2·0
Av.	88·0	73·0	7·7	42·6	4·0	2·5	3·3	3·0	3·2

(see Table IV). That is to say, there was nothing phenomenal or unusual about the accuracy and steadiness of the proof-readers' voluntary movement and fixation; but they did tend on the whole to be among the steadiest and most accurate.

IV. Conclusions.

The general conclusions seem, then, to be that good proof-readers possess a fair amount of accuracy and steadiness of eye movement, which is presumably innate because they had had no practice in making voluntary eye movements and fixations such as those just mentioned. This is coupled with considerable regularity of eye movement in reading; but it is not possible to say exactly how far this is natural or acquired. It is not acquired from much practice in normal reading; this is shown particularly by Subjects *C*, *D* and *E*, who had all had a great deal of practice in reading, but showed very variable reading times and many regressions. On the other hand, it does seem possible that regularity is acquired by much practice in proof-reading; since Subject *Z*, who had only been proof-reading a short time, made a good many more regressions than Subjects *W*, *X* and *Y*. It is probable that the regularity was only in part a habitual motor tendency. Both with the normal reading material, and the material containing misprints, it resulted to some

extent from lack of interest in, or apprehension of, the general meaning of the content, as did Subject *B*'s regularity in reading the normal material. This brings us back to the theory originally propounded, that ability in proof-reading is largely a matter of attitude or mental 'set' towards the material read; and that, while other readers can assume it to some extent, the professional proof-reader has perfected it by long practice, and can assume and maintain it automatically. Thus one proof-reader stated that in ordinary reading for pleasure he might or might not notice any misprints in the text. But when proof-reading he felt quite different; he "got down to it," and was confident that he did not pass over any misprint. The proof-readers had not, of course, had any practice in introspection, and were unable to describe this mental 'set' at all clearly. It seems probable that it involved a species of 'side-tracking' of assimilation of the general meaning of the content, coupled with alertness and scrutiny of typographical detail. That is to say, the words and letters were perceived more as a series of designs (just as, in glancing at a book of Euclid, one might perceive and name 'triangle, circle, right-angle,' and so on); but the symbolic associations of the words were of secondary importance in consciousness, while the whole complex of associated thought and imagery which usually follows word perception was almost entirely inhibited. This method of reacting to the printed words had become almost as habitual to the proof-reader as had the converse method to the mature and practised normal reader.

It may, perhaps, be objected that there is little experimental basis for such a theory. It was shown, however, that the proof-reader reacted to material containing misprints with a type of objective behaviour which was much more systematic and methodical than that of the ordinary reader; and that this type of behaviour, once initiated, persisted over a period when no misprints were encountered—that is to say, it was relatively permanent, and not quickly abandoned. Moreover, the subsequent introspective reports seemed to show that the proof-readers were much less muddled and upset by unexpectedly meeting with the misprints. They were able to carry out the rudimentary assimilation of the meaning which, it was noted above, was necessary for the detection of errors of grammar, contradictory statements and so on; whereas the ordinary readers had very little idea of any meaning at all when the number of misprints was large. Thus it is clear that the proof-reader possessed a well-adjusted habitual mode of response to the presence of misprints; and this mode did not show any objective characteristics markedly different from those of normal reading. It is also reasonable to

suppose that this mode of response involved a withdrawal of interest and attention from the meaning of the content. This was shown both by the fragmentary introspections of the proof-readers themselves, and by deduction from the fuller introspections of Subject *B*, whose procedure in reading normal material was so very similar to that of the proof-readers. Thus we may conclude that the mode of response to the mis-printed proof consisted of some such attitude or mental 'set' as that described above. It is still impossible to determine the extent to which this attitude is based upon underlying general intelligence or temperamental disposition. It seems fairly clear that there is no specific proof-reading ability, but that this aptitude must be related to a number of broad general traits of character. It also seems probable that natural accuracy of voluntary eye movement may be of assistance to the regular movement processes characteristic of the proof-reader.

V. Summary.

1. The eye movements made by four proof-readers in reading normal material and material containing misprints were compared with those of five other readers who had had no training in proof-reading.

2. The average reading time per line and the number and duration of the fixation pauses of the proof-readers were much the same as those of the other subjects in normal reading; but the standard deviations of reading time and number of pauses and the number of regressions of the former were considerably less than those of the latter.

3. In reading material containing misprints, the reading time per line and the number of regressions increased much less for the proof-readers than for the other subjects, but this increase was maintained by the proof-readers, and not by the other subjects, during the subsequent reading of material containing no misprints.

4. It was concluded that the proof-readers were able to adopt and maintain indefinitely an attitude or mental 'set' towards reading material containing misprints such that the recognition of small details of the structure of letters and words occupied the centre of consciousness, while assimilation of the general meaning of the content was relegated to the background. This accounted for the regularity of their eye movements. Non-proof-readers could not adopt this attitude with the same efficiency and permanency.

5. The proof-readers were naturally fairly accurate and steady in the execution of voluntary movements and fixations.

In conclusion I wish to express my thanks to the Cambridge University Press for its great kindness in allowing four proof-readers to act as the subjects of these experiments. My acknowledgments are due to the Medical Research Council, by whose auspices this work was made possible.

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APPENDIX.

The whole descent is like a dream to me, so rapidly was it accomplished. I had scarcely left the summit ere the valley had closed round my path, and the sun beat upon me, walking in stagnant lowland atmosphere. I was in different country from the day before; The stony skeleton of the world was here vigorously displayed to sun and air. The slopes were steep and changeful. Oak-trees clung along the hills, well grown, weathy in leaf, and touched by autumn with strong and luminous colours. Here and there another stream would fall in from the right or the left, down a gorgeof snow-white and tumultuary boulders. The river in the bottom (for it was rapidly growing a river, collecting on all hands as it trotted on its way) here foamed a white in desperate rapids, and there lay in pools of the most enchanting sea-green shot with watery browns. As far as I have gone, I have never seen a river of so delicate and change ful a hue; crystal was not more clear, the meadows were not by half so green; and at every pool I saw I felt a longing to be out of these hot, dusty, and material garments, and bathe my naked body in the mountain air and water. all the time as I went on I never forgot it was the Sabbath; the stillness was a perpetual reminder; and I heard in spirit the churchbells clamouring all over Europe, and the psalms of a thousand churches.

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AN EYE FACTOR AFFECTING PROFICIENCY AT BALL GAMES.

By H. BANISTER and J. M. BLACKBURN.

(*From the Cambridge Psychological Laboratory.*)

I. The 'good eye' not a true eye factor.

A 'good eye' is usually considered to be all important for proficiency at such games as polo, tennis, cricket, fives, etc., in which a fast-moving ball has to be hit with speed and precision either by the hand itself or by an instrument held in the hand. But, provided the individual can see, the 'good eye' appears to be quite independent of visual acuity. Many of the best players have an acuity far below the normal. In all probability the 'good eye' is not a true eye factor at all. It seems rather to be a very high innate visuo-muscular co-ordination, which enables the one who possesses it to hit the ball with his racket or bat held so that the plane of the face makes a particular angle with the direction in which the ball is travelling, the angle being determined by the way in which the ball is moving, and it enables him to hit the ball in exactly the right position in space and at the correct speed, with the time judged to an extraordinary degree of nicety. As is well known, all this may be accomplished by a man with very poor (uncorrected) visual acuity.

II. True eye factors are possible.

Yet it seems strange if no lower order visual factor than that entailed in this 'good eye' co-ordination is of importance: and much time has been spent in the attempt to find such elementary factors.

III. One such factor the inter-pupillary distance.

So far only one of the elementary factors tested by us has given positive results, viz. the inter-pupillary distance (I.P.D.). The probable

reason why this distance is of importance is that the wider apart the eyes the greater will be the differences in the disparation on the two retinae, with a consequent increase in the ability to judge the relative distances of objects. The individual whose eyes are close together approximates to the one-eyed man whose comparative inability to judge relative distances is well known.

IV. THE RECORDS.

The I.P.D.'s of 258 Cambridge undergraduates were measured to the nearest millimetre by means of a Zeiss Augenabstandsmesser, and each man was classified according to his ability at games. As always, this classification was most difficult. To obtain it each man entered on a prepared form his name, college, length of residence at Cambridge, and school last attended; he also stated his achievements, both at school and at the University, in the various games and pastimes. These statements were then gone through by a committee of four undergraduates, who decided into which of four classes each individual should be placed. The 1929 classification was made by each judge separately, without any reference to the other judges and the results were pooled, more weight being given to the judgments of those who knew the men personally than to the judgments of those who were going by the paper records only. The 1930 classification was made by the judges in collaboration.

Such classifications are manifestly inaccurate, but they are probably as accurate as can be obtained. They are inaccurate because the judges had never seen many of the men play, and even when they had seen them play their judgments were subject to the usual bias and 'halo' effects. Also good players in large colleges such as Trinity and St John's often experience difficulty in getting into their college sides, and so are neither known nor have any achievement to record at the University, whereas much poorer players from the smaller colleges find it difficult to escape from playing games, even if they so wish, and so tend to be put into a higher grade. In addition, a large number of men row, while others run or walk for exercise because they find that their studies are in that way less liable to interruption than if they play organized games. Such men, though they may be potentially good players of ball games must, according to these methods of classification, be ranked low.

Difficulties of these kinds are inherent in all enquiries into relations between attributes where the classification for one attribute has to be assessed, and militate against any relation being found.

The I.P.D.'s varied from 57 to 73 mm., the mean being about 64·5 mm.

Since the distances were measured to the nearest millimetre only, those men with an I.P.D. of 65 mm. or over were classified as above the average, those with 64 mm. or under as below the average.

The classification of these men in two groups, as Poor or Good players was:

I.P.D. (mm.)	Games ability: Poor	Games ability: Good	Total
up to 64	78	44	122
65 and over	67	69	136
Total	145	113	258

Statical analysis according to the χ^2 method shows that the probability of the distribution being due to chance is 1·8 per cent. In other words, the odds are 55 : 1 *against* chance.

Looked at from another point of view one may ask whether the I.P.D. of the Good player is greater than that of the Poor player on the average; and, if so, whether the difference is statistically significant. The answers to both these questions are in the affirmative. The mean I.P.D. of the 113 Good players is 64·946 mm.; that of the 145 Poor players is 64·157 mm. This gives a difference of 0·78 mm. on the average between the two groups. The standard error of the differences is 0·3517 mm., so that the difference of the means/standard error of the difference of the means = 2·24. Put in another way this means that the odds *against* the difference being due to chance are 39 : 1.

It therefore appears that one factor, though a small one, in goodness at ball games is the inter-pupillary distance.

V. Summary.

The inter-pupillary distances of 258 Cambridge undergraduates have been measured, and the measurements have been compared with their goodness at ball games. Analysis of the results shows that those with the greater distance between the pupils are on the whole the better players. This is probably due to the better stereoscopic vision which the greater width makes possible.

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INDIVIDUAL DIFFERENCES IN THE PERFORMANCE OF A SIMPLE TEST.

By J. M. BLACKBURN.

(*From the Cambridge Psychological Laboratory.*)

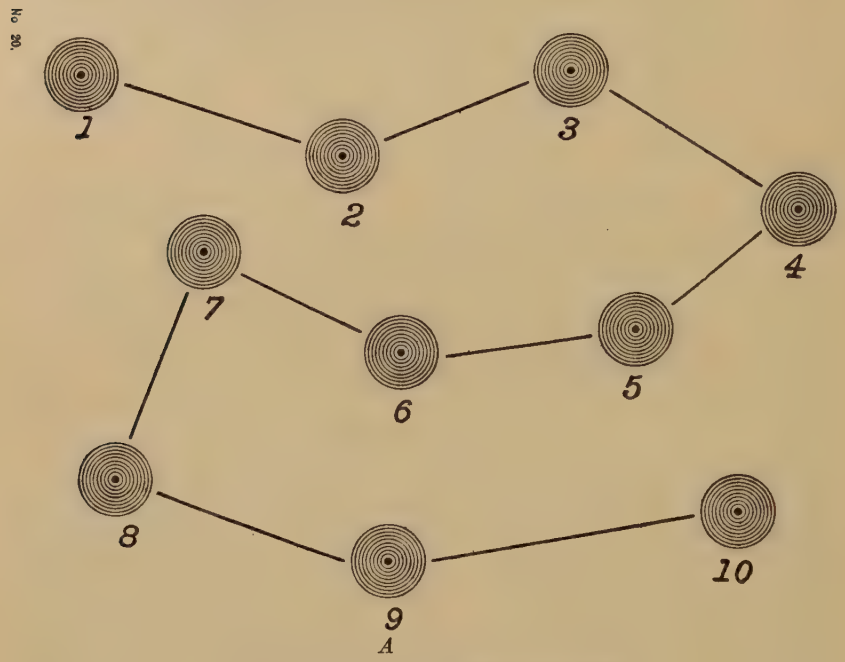
I. Introduction.

It is the aim of many psychologists to obtain psychological tests that will lend themselves satisfactorily to purely objective methods of marking, and tests to discover the presence or the absence of particular qualities, or the degrees to which these qualities are present or absent, are frequently invented with this aim in view. For a large number of reasons it is difficult to devise suitable tests. It is, for example, extremely difficult properly to analyse the different qualities required in the performance of a given operation; it is difficult to know what differences there will be between abstracted specific qualities and qualities which are combined with other qualities in a test; it is difficult to be sure that a given test is truly analogous to the operation in question; and it is difficult to know that a test will be a true sample of the work which it is designed to test, when it is given in conditions which are artificially made to resemble natural conditions.

When these difficulties have been satisfactorily settled, and conveniently simple tests have been devised, there remains another difficulty of equal importance. Subjects very often perform the same test in different ways, so that a uniform system of marking is not appropriate. It is the object of this paper to show the large range of individual variation which can occur in the performance of a comparatively simple test, and to urge the desirability of combining subjective estimates with objective methods of marking.

II. Description of experiment.

The experiment was the ordinary Aiming Test with targets arranged as shown in the diagram below:



Target form reduced in size by one-half

Fig. 1.

Figures have been inserted under the targets in the diagram to simplify the discussion which follows; there were no figures under the targets on the form used in the experiment. The 128 subjects were army recruits of less than three months' service. The subjects held a metal pointer in their hand and kept time to a metronome set at 112. They tried to spear the centre of each target in turn, following the targets round in the order indicated by the numbers in the diagram until they reached the end, when they began over again. The subjects did not go direct from target to target but at every alternate beat of the metronome they returned to the starting position at *A*. Thus on the first beat of the metronome they speared target 1, on the second they returned to *A*, on the third they speared target 2, on the fourth they returned to *A*, and so on. Although there were no figures under the targets it was pointed out to the subjects that there was a line which connected each target with

another, so that they could see quite easily which was the next target they were to spear.

The preliminary instructions were almost identical, and the experimenter performed the test himself in front of the subject. The subject then practised it and the experimenter corrected any mistakes he made. When the subject had performed two rounds in succession without making any mistake it was assumed that he had learnt the test. An unused target form was then set in front of him and he continued the test until told to stop. At the end of five rounds the order to stop was given.

It is usual to mark the test by measuring the distance of each prick made from the centre of its respective target.

It might be thought that a fair proportion of the subjects would readily understand the instructions so as to perform the two practice rounds and the five test rounds straight away without making any mistake, except in the accuracy of their aiming. It might also be thought that almost all, though perhaps making mistakes in the practice rounds, would eventually perform the actual test in the correct manner. However, 35 subjects (27·3 per cent.) performed the practice rounds as well as the test itself correctly; 35 subjects (27·3 per cent.) performed the test correctly, though they made mistakes in the practice rounds; the rest, amounting to 58 subjects (45·3 per cent.), made mistakes in the test after they had apparently understood how to do it.

The following is a list of the mistakes that were made in each round. The numbers indicate the order in which the targets were speared. It is advisable to refer constantly to the diagram of the target form in order to appreciate properly the significance of each mistake that was made.

III. Discussion of the mistakes.

Table I represents only the order in which the targets were speared in each round separately. A further analysis was made of the mistakes made by each subject in the five rounds taken together. A part of this analysis is as follows:

In 19 cases, *i.e.* in the records of 19 subjects, the **765** mistake was made by itself, without being combined with any other mistake in any other round. In 9 of these cases the **765** mistake was made in only one round, in 7 cases it was made in two rounds, in 1 case it was made in three rounds and in 2 cases it was made in all five rounds. In 9 cases the **765** mistake was combined with other mistakes, such as the **7651098** mistake, in other rounds.

In 1 case the **1098** mistake was made by itself. In 3 cases the **1098** mistake was combined with other mistakes in other rounds.

Table I. Analysis of mistakes.

Mistakes	Numbers making mistake in each round				
	Round 1	Round 2	Round 3	Round 4	Round 5
12347658910	16	11	8	7	9
12347651098	2	5	2	1	4
12345671098	4	2	2	1	1
12345678109	1	1	2	1	1
12347651089	1	.	.	.	.
1234765981010	.	.	.	1	.
12347659810	.	.	.	.	1
1234765108	.	.	.	1	.
123476548910	.	.	.	1	.
1234*678910	1	1	1	.	1
1234*78910	.	1	.	.	.
12345*78910	.	1	1	.	.
1234567*910	.	.	.	.	1
123456789*	.	.	.	.	2
1*347658910	1	.	.	.	.
1234*67	.	.	.	1	.
12347*58910	.	.	.	.	1
123410678910	1	.	.	1	.
123465678910	1	.	.	.	.
1234105678910	.	1	.	1	.
132345678910	.	.	1	.	.
123456788910	.	.	.	1	.
123475678910	.	.	.	.	1
12347678910	.	1	.	.	.
12398735	.	.	.	1	.
1234656*8910	.	.	.	.	1
12341056*8	.	.	.	.	1
12345681098	.	1	.	.	.
12341	.	.	1	.	.
12345697810	.	.	.	.	1
91056789	.	1	.	.	.
112223344765889910	.	.	.	.	1
98765104321	1	.	.	.	.
12341098	1	1	1	1	.
1234108910	.	.	1	.	.
12341098789	1	.	.	.	.
12345109	.	.	.	.	1
34127651098	.	1	.	.	.
3541010987654321	.	.	1	.	.
12341095687	.	.	.	1	.
8911476655888991010	.	.	.	1	.
12345109687	1	.	.	.	.
123465108910	1	.	.	.	.
12354109687	.	1	.	.	.
469871682734109	.	.	1	.	.
1234567/8910	.	1	.	.	.
123456789/10/9	.	.	1	.	.

The last two mistakes were made by proceeding direct from one target to the next at the points marked '/.'

* Indicates that the intervening targets were omitted.

In 4 cases the **7651098** mistake was made by itself in one or more rounds.

In 5 cases the **7651098** mistake was combined with other mistakes in other rounds.

In 2 cases the **8109** mistake was made by itself.

The other mistakes for the most part appeared by themselves, but at other times they were combined with other non-recurrent mistakes, the extreme example being the record of one subject who, although he had performed the practice rounds twice in succession correctly, gave the following record: 12345**109687** in the first round, 12**354109687** in the second, 12**3541010987654321** in the third, 12**341098687** in the fourth, and 12345**109** in the fifth.

Two important points about the mistakes that were made are:

(1) That in the vast majority of cases the errors occurred after the subjects had reached the end of a line, *i.e.* after they had speared either target 4 or target 7; and

(2) That the errors chiefly consisted in spearing all the targets *in a particular line* in the wrong direction.

It is suggested that these two facts indicate that the target form tended to be perceived by the subjects as composed of three Gestalten, each of which consisted of one particular line of targets. The mistakes that were made are more easily understandable on this assumption, which is not, however, elaborated any farther here.

The commonest mistake was the **765** mistake. Instead of spearing the targets in the correct order, the subjects in this group speared only the first four targets and the last three targets in the correct order, but reversed the correct order of the three middle targets. This mistake, like the other mistakes that were made, was not made consistently. It would appear in one round and disappear in the next. In a fair number of cases not counted as mistakes a tendency to commit this error could be observed. The subject would spear target 4 and then hesitate before proceeding to target 5. These hesitations are mentioned to show that the potentiality for making the mistake was greater than the figures actually indicate. The error itself is easily understandable. In writing we proceed in each line from left to right. In this test the position of the hands is the same as in writing and the right hand grasps a pointer that is similar to a pen or pencil. The threshold for the writing 'pattern' in the mind is therefore very low. Consequently, if the attention of the subject be relaxed during the experiment, part of the writing pattern may easily be called into operation. This, too, will explain the hesitation of some subjects at target 4.

The second mistake which was made, though not nearly so frequently,

appears at first sight to contradict the explanation that has been offered for the **765** mistake. This was the **1098** mistake. In 9 cases it was combined with the **765** mistake in the same round, and in these cases it has been scheduled as *one* mistake, viz. **7651098.**

In the **1098** mistake the subject proceeded from right to left in the bottom line of targets instead of proceeding from left to right. An explanation of the mistake in the cases where it was combined with the **765** mistake might be that the subject remembered, while he was performing the second line of targets, that he ought to proceed from right to left in one of the lines, with the result that he proceeded from right to left in the third line. This suggested explanation lacks proof. Introspections were, unfortunately, not obtained from the subjects for the reason that the test was not used primarily for this study of individual differences.

The **8109** mistake was made by one subject in all five rounds. The explanation of this may be that the subject perceived the target form as composed of four Gestalten, target 9 which is on a lower plane than targets 8 and 10 being perceived as a distinct unit. This explanation is not entirely satisfactory because, for instance, target 2 is on a lower plane than target 3 and yet targets 1, 2, 3 and 4 were speared in the correct order.

One subject performed the **41098** mistake in the first four rounds and in the fifth round he speared the targets in the order 123**410568910**, suddenly introducing the middle line of targets into his record.

One subject made the mistake 123**410**678910, *i.e.* the substitution of target 10 for target 5, in rounds 1 and 4, but he performed the other rounds correctly.

All the other mistakes, including the omissions, substitutions and additions of targets, were made by the subjects in only one round of their test while they performed all the other rounds correctly.

IV. Discussion of methods of marking.

The first question that arises is whether all the records together can be regarded as forming a homogeneous group. If the answer to this question is in the negative, what will be the best method of treating the records of the 58 subjects who made mistakes, so that they can be compared with those of the other subjects?

If it is required to obtain from the results a curve which will give the distribution of accuracy in the test, a curve from which can be drawn the

mean, median and modal scores together with the standard deviation of the distribution, then in the first place the records of all the subjects can be included provided that all errors except simple accuracy of aiming tend to cancel one another out in the long run. In the second place, if all errors except simple accuracy do not tend to cancel one another out, all the records can still be included, provided that some reliable system of weighting the different mistakes so that they can be artificially made to cancel one another out can be evolved.

An examination of the errors made in this test shows that neither of these conditions is satisfactorily fulfilled. There are four groups of mistakes, viz. the **765**, the **7651098**, the **1098** and the **8109**, which tend to recur, but there are other mistakes which occur only once among all the records. There are two cases in which a subject added a target in one round and omitted a target in another round, but apart from these two cases there is no indication that the subjects who made one mistake in one round made a compensatory mistake in another. Even if there were it would be extremely questionable to assume that the omission of a target in one round was psychologically equivalent to the addition of a different target (or even the same target) in another round.

Furthermore, there is no satisfactory method of weighting the different mistakes. An inspection of the table shows that the non-recurrent mistakes cannot be regarded as of equal importance (for instance **1122233447658899l0** cannot be regarded as of equal importance to **468871682734109**), and there is no method of discovering their relative psychological importance. With regard to the recurrent mistakes the table shows that the four recurrent mistakes, viz. the **765**, the **7651098**, the **1098** and the **7651089** are made in the five rounds by the following numbers of subjects respectively:

Round 1	16	2	4	1
Round 2	11	5	2	1
Round 3	8	2	2	2
Round 4	7	1	1	1
Round 5	9	4	1	1

That is to say that the relative frequency with which they occur varies in each round.

The result is that no standard method of weighting can be evolved either for the recurrent or for the non-recurrent mistakes.

Consequently, either all the 58 records which show mistakes must be discarded or else the attempt to obtain an objective classification of the

subjects as regards their accuracy of aiming must be abandoned. If the former position is adopted there are still further difficulties to be encountered. There are other individual differences to be taken into account. Such are the relative ability to keep time with a metronome correctly, differences in the amount of practice which each subject needs before he learns how to do the test, differences in the degree of hesitation shown by some subjects before or after they speared a target, and also the different targets at which this hesitation occurred. These are differences which it is difficult to measure correctly (in the case of hesitation it is almost impossible), but even if they are measured and weighted correctly yet it must be remembered that the introduction of artificial, objective measurements will mask interesting individual subjective differences.

V. Conclusion.

There remains the possibility of abandoning the attempt to obtain a purely objective measurement of accuracy in aiming. In this case the group method of applying the test must be discarded. Every record must be treated individually and a careful note made of the different ways in which each subject performs the test. The amount of practice that he needs before he has learnt the test; his persistence in making mistakes of different kinds in the practice rounds; his suggestibility to correction; his sense of rhythm; his natural speed of movement; his interest or boredom in the experiment; the nervousness with which he approaches it; his annoyance or indifference when he makes mistakes; all these qualities are observable if the subject is consistently watched. These observations, which should be combined where practicable with the subjects' introspections, are of great importance. They can be used with advantage to supplement each objective record before a final ranking of the different subjects for accuracy in aiming is decided upon. Vernon[1] has shown the need for making subjective estimates of temperamental qualities, and this paper attempts to show that tests which do not lend themselves completely to a system of objective marking are to be encouraged rather than discarded. They have a definite usefulness, and there is considerable scope for the further investigation of psychological characteristics along the line of a combination of subjective estimates with objective methods of marking.

[1] P. E. Vernon, *Brit. J. Psychol.* 1929, xx, 97–117.

VI. SUMMARY.

1. The mistakes of 58 out of 128 subjects in performing five consecutive rounds of the Aiming Test are analysed (with the exception of individual differences in simple accuracy of aiming).

2. There are four groups of mistakes which tend to recur, though the relative frequency with which they occur differs from one round to the next.

3. There are many mistakes of varying degrees of complexity which are non-recurrent.

4. No satisfactory estimate of the relative importance of the different kinds of recurrent and non-recurrent mistakes can be obtained.

5. For a purely objective method of marking the subjects for accuracy in aiming, all the records of the 58 subjects who made mistakes must be discarded. This leads to a consideration of the rejection of other records for other reasons.

6. If the objective method of marking be supplemented by subjective estimates of various qualities possessed by the subjects, the records of all the subjects can be retained.

I wish to thank Dr H. Banister for the idea of writing this paper, and to Professor Bartlett I am indebted for valuable criticism.

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AN EMPIRICAL RESEARCH ON THE RELIABILITY OF CORRELATION COEFFICIENTS.

By J. L. PRAK.

I. Introduction.

The first attempt to throw light upon the reliability of correlation coefficients for small numbers of observations was made in an article published in *Biometrika*, VI, 1908–9, pp. 302–10, entitled "The Probable Error of a Correlation Coefficient." The author calls himself 'Student.' The material of which he made use was provided by measurements of (*a*) the stature, and (*b*) the length of the left middle finger of 3000 criminals. The correlation between these two was 0·66[1]. 'Student' divided his material into 750 groups, each of four persons, and by combining each group of four persons (i) with the tenth one before, and (ii) with the tenth one behind it, he obtained a further series of 750 groups, each of eight persons. He likewise determined the correlation coefficient for a series of 100 groups, each of thirty persons. These records he published in the form of a table.

'Student' then proceeded to compare the figures which he had thus obtained empirically with those which would be expected in accordance with mathematical formulae. He experimentally determined whether these formulae held for groups of four, eight and thirty cases, and he came to a negative conclusion for the first two cases, but to a positive one for the last one.

Applying to these conclusions the method I have used in my own study (which was necessarily on a much smaller scale), the results may

[1] Correlations in this article are calculated according to the formula of Bravais-Pearson

$$r = \frac{\Sigma xy}{\sqrt{\Sigma x^2 \Sigma y^2}}.$$

be thus expressed. If, in groups of from four to thirty cases, the results were exactly the same as in one group of 3000 cases, one would expect in the cases of groups of four, eight or thirty persons a constant correlation 0·66 between stature and the length of the left middle finger. In point of fact this is no more true than if one claimed to be able to deduce the average stature of a population of 3000 persons from the stature of four, eight or thirty persons, or the school marks of a whole town from the school marks of one class.

In the probable error we have a measure for the expected deviations from the real correlation. As is well known, the probable error (which is 0·67449 of the standard variation) signifies something far more precise than its name might convey: it signifies the quartile of a normal frequency distribution; in other words, it means that there is as much chance that a value lies within the limits of the probable error as outside them. So, if we find a correlation $0{\cdot}50 \pm 0{\cdot}04$, this means that with a great number of correlation-calculations for equal groups, 50 per cent. of the correlation coefficients will be between 0·46 and 0·54. Thus the probable error gives a measure for the spreading of the correlation coefficients. If the formula $\text{P.E.} = \pm 0{\cdot}6745 \frac{1 - r^2}{\sqrt{n}}$ still also holds for thirty, eight or four cases, then every time 50 per cent. of the correlations should be $0{\cdot}66 \pm \text{P.E.}$ But 'Student's' examination, described above, showed that with groups of four, eight and thirty cases 39, 43 and 58 per cent. respectively were within the limits. So in groups of four and eight cases the variation was greater than would have been expected according to the formula, and was smaller with groups of thirty. In other words, with such small groups as those of four and eight, one must reckon with a greater probable error than is given by the above-mentioned formula; that is to say, in the comparison between stature and length of the left middle finger; with groups of four and eight cases the probable error needs to be about 10 per cent. greater to contain 50 per cent. of the correlations, while in groups of thirty cases it is about 10 per cent. less.

However instructive this study may be, for our special object it falls short in two important respects. In the first place, the examination deals with physical phenomena; and although it is very probable that mental phenomena show similar variations and follow similar laws, we are not entitled without further inquiry to build upon such an analogy. For widely as the coefficients of variation for physical phenomena vary, those for the results of psychological tests fluctuate still more. Further, in a psychological research we have to reckon with errors of observation of a

kind entirely different from those which occur in the afore-mentioned measurements. The results of psychological measurements depend on the clearness of instruction, the understanding of the subject, the degree of training, concentration of attention, competition, accidental interruptions, illumination, etc. Therefore in considering the results of a psychological test, we have to consider not only the errors of random sampling, but also the deviations inseparable from the very nature of the research.

In the second place, the number of groups employed by 'Student' suddenly leaps from four and eight to thirty: he neglects sizes which are in quite common use in applied psychology (ten to twenty). If we have recourse to interpolation in order to bridge this gap, we desert the empirical method.

II. Description of method and tests used.

In order to solve these difficulties, the following method was adopted. The correlation was determined between a number of tests and the total scores of a series of other tests, for an entire group of 152 subjects as well as for groups of thirty, twenty, fifteen, ten and six subjects. In this way, it was possible to examine empirically how far such small groups still gave a reliable correlation coefficient. As, besides the errors of random sampling, those deviations which have their cause in the nature of the examination are important, a short description of the tests and of the attending circumstances is here desirable.

The six tests, which were chosen for this research, were the following: correction of words, formation of words, analogies, intelligent memory, continuation of patterns and visual memory. The second series of tests, the *total* scores of which were correlated with the score of each test in this first series, consisted of another twelve tests of theoretical and practical intelligence, together with some occupational tests. All the tests in the two series were given in the entrance examination for the Eindhoven Trade School. (They are more closely described by me in *Mededeelingen van de D. Bos Stichting*, No. 13, Het Psychotechnisch Onderzoek aan de Ambachtsschool te Eindhoven, J. B. Wolters, Groningen, 1926.) To the results of these tests, with which the six above-mentioned tests were compared, marks were given according to importance (0–4, 0–8, 0–12, 0–16). By adding up the separate marks for each subject, a total result was obtained, which gave one a clear enough notion of the candidate's general suitability for a technical trade. The following is a detailed description of the six selected tests.

Correction of words. The examiner wrote the word 'dream' on the

blackboard (*droom* in Dutch). Underneath he wrote words, formed out of the letters of 'dream,' like: 'am,' 'me,' 'dam,' 'are,' 'red,' etc. (*moor, oom, rood, moord*, etc. Dutch). Finally he wrote: 'dead' and 'rod' (*dam* and *dood* in Dutch). The boys were asked whether the words 'dead' and 'rod' could be formed out of the letters of 'dream.' When this question was answered in the negative, the two words in question were demonstratively crossed out, while the examiner asked why this was not possible. Thus, before starting, stress was laid on the character of probable mistakes, using a wrong letter, using the same letter twice, etc. Then the paper, which had been laid face downward on the table, was allowed to be turned. On this paper were two columns of words. At the top of the first column was the word 'spring-green.' The following instructions were given. "Now look at each word printed underneath, and see whether it can be formed out of the letters of 'spring-green.' If it can, leave it as it stands. But if it cannot, cross out the word, just as I did the words 'dead' and 'rod' under the word 'dream.' Ready—go"! Eight minutes were allowed for considering 120 words. When a number of boys drew near to the second column, the examiner said that the words in this one should be treated in the same way, but having regard to the new word standing at its head. The average result was 78·75 words; maximum 120, minimum 37, with a standard deviation of 18. For brevity's sake, I shall henceforth denote these by av., max., min. and σ.

Formation of words. Of three words, for which respectively two, three and three minutes were given, the following total numbers of words were formed: av. = 22, max. = 47, min. = 0, $\sigma = 6{\cdot}97$.

Analogies. Fifty analogies were to be sought: The numbers of correct completions were: av. = 15·13, max. = 39, min. = 0, $\sigma = 7{\cdot}83$. The numbers of wrong completions were: av. = 4·79, max. = 21, min. = 0, $\sigma = 4{\cdot}03$.

Intelligent memory. After a detailed example, ten groups, each composed of three coherent words, were slowly spoken. About half-an-hour afterwards this was repeated with another ten groups. The examiner gave the first word of each group in the original order, and the subjects had to write down the whole row of three words. With forty correct reproductions as maximum, we got the following results: av. = 20·62, max. = 38, min. = 0, $\sigma = 9{\cdot}65$.

Continuation of patterns. A number of five simple patterns, resembling plain border-decorations, had to be continued. Marks for each pattern were given from 1 to 5. The results were: av. = 12·26, max. = 21, min. = 5, $\sigma = 3{\cdot}43$.

Visual memory. Nine pictures were shown one by one for 8 seconds. The subject's reproductions of these were scored according to a scale of 5 marks. The absolute maximum was 45 marks, the results were: av. = 24·92, max. = 41, min. = 11, $\sigma = 6{\cdot}25$.

III. Experimental procedure.

My 152 subjects were boys of 12, 13 and 14 years old. Seventeen boys had reached the fifth class of the grammar school, ninety-five boys the sixth, thirty-seven boys the seventh class, and three boys the eighth class.

The examination took place in groups of about thirty (twenty-eight to thirty-six), after an entrance examination in grammar and arithmetic held some days before. The duration of the examination was about five hours—from 8.30 to 11 o'clock in the morning in an ordinary classroom, and from 1.30 to 4 o'clock in the afternoon in a large arts' room, with short intervals in the two work periods.

I began with a questionnaire which had to be filled in. The selected tests were distributed over the whole day, forming respectively the sixth, first, fifth, thirteenth and seventeenth, eighth and nineteenth paper. The examiner was the same throughout the examination; so was his assistant. The examination was carried out under conditions as far as possible identical for each group, with the same examples and instructions, similar encouragement, etc. Some days before the examination a rehearsal was carried out on a small group of five boys (who did not take part in the examination) in order to make sure of a satisfactory standardization of conditions. Despite these precautions the examination did not prove to be so uniform as a series of tests daily repeated over a long period, or as the army intelligence tests where the instructions are mostly written on paper.

IV. Factors influencing the reliability of the results.

The factors which influenced the reliability of the results may be divided into disadvantageous and advantageous ones. Among the latter, I may mention the homogeneity of the subjects, as regards their age, the constancy of time, testing-rooms, examiner, and of the incentive to effort (*i.e.* keenness to be admitted to the school, only half the number of applicants could be accepted). Among the disadvantages may be cited the comparative newness of the series of tests; the considerable differences in school training of the boys, which was not so much due to the number of different classes as to the great difference in quality of education (for further details see the above-mentioned pamphlet); the insufficient

suitability of the class-rooms for a psycho-technical examination, compared with a room, specially equipped for it; the differences in illumination (sunshine, cloudy sky, distance from the window) and in distance from the examiner and from the blackboard. As a matter of fact, the boys with defective vision were given a place in the front row. Copying from each other was absolutely prevented in the afternoon and fairly well prevented in the morning session.

All things considered, the circumstances were neither more nor less favourable than they would be in a normal test examination in industrial psychology, and we can very well accept our results in this case as being normal, although they were not obtained under optimal conditions.

V. Discussion of experimental results.

The correlations of the six separate tests with the criterion (total scores) upon which the tests have no influence themselves, were + 0·187, 0·440, 0·515, 0·528, 0·628 and 0·659 respectively. I expressly chose tests which correlated in a very divergent way with the criterion, in order to find for low and average, as well as for high, correlations the reliability of correlations with small groups. For 152 subjects the probable errors of these coefficients were ± 0·054, 0·044, 0·04, 0·039, 0·033 and 0·031 respectively. In order to obtain the smaller groups, the total scores and the results of the six tests for each of the 152 subjects were written on a slip of paper, and out of this material six groups of thirty, twenty, fifteen, ten and six were drawn. For the six groups of 30, a second drawing out of the total number was necessary, the others could be gathered at once. The correlations of these thirty groups were as follows: the probable error $\left(\text{according to the formula P.E.} = \pm 0{\cdot}6745\frac{1-r^2}{\sqrt{N}}\right)$ for the correlation coefficients of the total group are placed beside them, together with the average of the six correlations.

If the correlations of the small groups had been in exact agreement with the formula, so that for every test and in every group-number 50 per cent. of the coefficients fell within the limits $r \pm$ the probable error, then in the above table we should expect a 3 everywhere, and there should be 15 everywhere in the right-hand column and 18 in the bottom row. And the total number would then be 90.

It is evident that for every test and in every group-number the actual agrees fairly well with the theoretical figure. There are, of course, fluctuations with such small numbers, but the total result of ninety-six instead of ninety inspires confidence. The one considerable deviation

Table I.

Six groups of thirty persons.

Tests							Av.	P.E. ±
Correction of words...	0·210	0·128	0·231	0·236	– 0·157	0·475	0·187	0·118
Derivation of words	0·602	0·523	0·227	0·451	0·449	0·579	0·471	0·099
Analogy	0·578	0·543	0·462	0·492	0·443	0·400	0·486	0·090
Intelligent memory	0·587	0·518	0·374	0·563	0·512	0·535	0·514	0·088
Continuation of patterns	0·645	0·587	0·575	0·691	0·712	0·695	0·650	0·074
Visual memory ...	0·709	0·732	0·698	0·590	0·496	0·645	0·645	0·069

Six groups of twenty persons.

Correction of words...	0·023	0·550	0·052	– 0·192	0·314	0·202	0·159	0·145
Derivation of words	0·506	0·583	0·380	0·327	0·199	0·409	0·400	0·121
Analogy	0·578	0·446	0·514	0·259	0·302	0·669	0·461	0·110
Intelligent memory...	0·593	0·322	0·500	0·592	0·460	0·605	0·512	0·108
Continuation of patterns	0·750	0·417	0·747	0·498	0·885	0·431	0·621	0·091
Visual memory ...	0·809	0·548	0·555	0·671	0·698	0·588	0·644	0·085

Six groups of fifteen persons.

Correction of words...	0·241	0·681	– 0·186	0·039	0·046	– 0·188	0·092	0·168
Derivation of words	0·512	0·620	0·797	0·599	– 0·289	0·476	0·452	0·140
Analogy	0·625	0·716	0·746	0·701	0·320	0·193	0·550	0·128
Intelligent memory...	0·830	0·579	0·600	0·471	0·475	0·466	0·536	0·125
Continuation of patterns	0·714	0·663	0·843	0·834	0·731	0·398	0·697	0·105
Visual memory ...	0·580	0·578	0·612	0·788	0·633	0·545	0·622	0·098

Six groups of ten persons.

Correction of words...	– 0·179	0·207	0·712	0·293	– 0·430	0·060	0·110	0·205
Derivation of words	0·372	– 0·015	0·639	0·841	0·587	0·116	0·423	0·172
Analogy	0·914	0·476	0·809	0·468	0·579	0·619	0·644	0·156
Intelligent memory...	0·596	0·763	0·840	0·537	0·733	0·423	0·648	0·153
Continuation of patterns	0·652	0·137	0·607	0·334	0·391	0·344	0·410	0·126
Visual memory ...	0·226	0·546	0·794	0·817	0·532	0·545	0·576	0·120

Six groups of six persons.

Correction of words...	– 0·114	0·594	– 0·271	0·759	0·404	0·194	0·261	0·266
Derivation of words	0·557	– 0·247	– 0·344	0·666	0·844	0·250	0·287	0·222
Analogy	0·442	0·310	0·086	0·621	0·645	0·352	0·380	0·203
Intelligent memory...	0·432	0·507	0·229	0·675	0·377	0·951	0·528	0·199
Continuation of patterns	0·870	0·945	0·725	0·639	0·886	0·909	0·829	0·167
Visual memory ...	0·207	0·764	0·740	0·506	0·762	0·721	0·616	0·156

Coefficients falling within $r \pm$ P.E.

Number in group ...	30	20	15	10	6	Total
Correction of words ...	4	3	2	3	2	14
Derivation of words ...	3	4	2	2	3	14
Analogy	5	3	1	4	4	17
Intelligent memory ...	5	5	4	3	4	21
Continuation of patterns	5	0	3	2	2	12
Visual memory ...	4	3	4	2	5	18
Total	26	18	16	16	20	96

(twenty-six) occuring in the group of thirty, is on the safe side. As regards the situations of the largest deviations, those which amount to two, three or four times the probable error, practice and theory agree again rather well.

With an unlimited number of correlation coefficients, it may be expected that one in six deviations will be greater than twice the probable errors, that one in twenty-three will be greater than three times, and one in 143 will be greater than four times the probable error. In regard to our 180 correlation coefficients, theory and practice conform as follows:

Table II.

Deviations		Correlations of						
Greater than	According to the formula	30	20	15	10	6	Total	According to formula
1 × P.E.	18	10	18	20	20	16	84	90
2 × P.E.	6	4	7	9	9	6	35	30
3 × P.E.	1·6	0	0	1	3	2	6	8
4 × P.E.	0·25	0	0	1	0	0	1	1·26
5 × P.E.	0·029	0	0	1	0	0	1	0·144

As regards the larger deviations, too, the coefficients fall with sufficient accuracy within the limits denoted by the formula.

VI. Conclusions.

We may conclude then, so far as the limited extent of this examination justifies the conclusion, that the formula for the probable error in our groups actually holds, although the groups of thirty and twenty persons give a more favourable result than those of fifteen, ten and six.

The number of deviations which is greater than twice the probable error is 50 per cent. more in the groups of ten and fifteen than would be expected according to the formula. Nine however of these eighteen large deviations are caused by the two lowest correlating tests, while three tests have only four of these large deviations and give therefore a better result than might have been expected.

Another question is: When is the correlation calculation still practically reliable? The deviations with ten and six persons approximately correspond with the formula, but the formula may also admit practically useless coefficients.

In reality, numbers which can vary 0·40 and more, are of little importance. We see, however, that the average of six groups of six approaches the real coefficient to less than 0·10 in three of the six tests, while only one average correlation deviates 0·20 from that of the total

group. In the six groups of ten, the maximum deviation of the average correlation is 0·218, but five deviations remain below 0·14 and three below 0·085. In the six groups of fifteen, the maximum deviation of the average correlation is 0·095 and the others remain below 0·07, while four deviate less than 0·04. In the groups of twenty the greatest deviation is 0·054 and three are smaller than 0·02. So we find, as 'Student' found, that a correlation from groups of thirty subjects is reliable, although repetition will strengthen this reliability. Roughly speaking, three coefficients out of four are fairly correct (their deviation does not exceed 0·07). In groups of twenty persons the reliability already diminishes, though it still corresponds perfectly to the formula for the probable error. Repetition of the examination with other groups of twenty is, therefore, still more desirable. With two or three groups the reliability is already fairly high; that is to say, reliability does not mean here that the result is absolutely correct and that it will not alter when the examination is extended, but a certainty which allows to judge whether a test for a special pedagogical or industrial purpose is useful or not and which makes it possible to determine roughly the total value of a series of tests for such a purpose. Groups of fifteen already show rather large deviations; only thirteen of the thirty-six coefficients deviate less than 0·10 from the real value and the number which deviates more than 0·20 is also thirteen. It is necessary to examine three or four groups of fifteen in order to get a reliable result. With groups of ten one should have at least six groups, and more than six with groups of six.

The average deviation of the thirty-six coefficients for each group from the 'real' value (with 152 boys) is for thirty, twenty, fifteen, ten and six respectively 0·077, 0·120, 0·176, 0·209 and 0·248. For the two tests with the greatest deviation (*i.e.* the lowest-correlating tests) these numbers are about 40 per cent. higher; while for the two tests with the smallest deviation (intelligent and visual memory) they are 31 per cent. lower.

Generally a number of at least fifty subjects is desirable before being able to speak with certainty. This does not exclude the fact that a careful examination of a small number of subjects followed by correlation calculation is the most reliable method and is to be preferred to subjective estimation of the relation between the variation series as is occasionally done by vocational psychologists. But such a limited material is insufficient to draw wide conclusions and the examiner will do well to expect large variations when eventually he repeats his examination and so make allowances for these in the practical steps which he may adopt.

In such a case it is preferable to spend money, time and energy on repetition of the examination rather than on more subtle calculations and methods of correlation. These latter do lead to higher correlations, it is true, but they do not always rest on an equally firm basis.

Another question is whether it is not possible to get the same results with a small number by appropriate selection from a larger group; *e.g.* by selecting, as regards the variation-series, a representative group. In the present instance those groups were selected which were uniformly constituted as regards the total score in the 12 tests; that is in regard to the criterion. Thus we selected two groups of thirty and twenty, four groups of fifteen and ten, and six groups of six persons, which were uniformly spread over the scale of the total score and we again calculated their correlation with the six tests. Results were not materially altered, neither for better, nor for worse.

Finally I have to offer my best thanks to Dr C. S. Myers for all the trouble he took in kindly correcting and polishing my rather bad English.

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THE COMPARATIVE PSYCHOLOGY OF JEWS AND NON-JEWS: A SURVEY OF THE LITERATURE.

By J. RUMYANECK.

(*Under the auspices of the Jewish Health Organization of Great Britain.*)

In view of the paucity of exact knowledge as regards the psychological differences between races, it is surprising to find each race endowed by anthropologists and sociologists with attributes peculiar to itself, and which are hereditarily determined. Thus, according to Crew in his *Inheritance in Man* the Nordic race is "prudent, self-controlled, its sensuous instincts subordinated to higher aims, courageous, sea-faring, athletic." The Mediterranean peoples, on the other hand, "are vivid, volatile, easy in self-expression, take life not too seriously and are not renowned for adherence to truth or loyalty." The Negro is "improvident, boisterous, or else dejected, lacking creative power, imagination and ambition, is cruel."

In the following essay an attempt is made to collate the judgments that have been passed on the Psyche of the Jew and especially on his intelligence, which is supposed to be superior to that of other peoples. The results of a number of intelligence and emotional tests that have been applied are critically examined, and the view is expressed that the claims of intelligence testers as being able to reveal innate psychological differences among different groups have in no way been substantiated; that although the Jews may be intellectually superior and may possess unique psychological faculties, no technique is in existence at present for their evaluation. The former trend in thought which ascribed to heredity or nature preponderant weight in the formation of these psychological differences is found to be untenable, and the view is suggested that in dealing with large groups of people as distinct from special family lines, environment is of overwhelming and pervasive importance.

Qualified observers in England of the Jewish community have always been struck by the intelligence of the Jewish children. C. Russell (*The Jew in London*, 1900) writes: "The foreign children in the East End are universally allowed to be sharper and more intelligent than the English, and they carry off a large proportion of prizes and scholarships." "I was astonished at their intelligence," said Sir C. Cameron before the

Inter-Departmental Committee on Physical Deterioration in 1904, and Colonel Fox thought that the Polish, Russian and Roumanian Jewish children were of higher intelligence than the English children. Two years previously witnesses before the Royal Commission on Alien Immigration expressed similar opinions. Mr G. L. Bruce, a member of the School Board in charge of the Whitechapel schools said: "The Jewish children have proved excellent scholars." Messrs J. M. Myers, S. Mather, F. H. Butcher and W. A. Nugent all thought that intellectually the Jewish child is superior to the non-Jewish child.

In 1916 in Germany O. Nemeck(1) analysed the scholastic records of 1549 pupils of a commercial school and found that the Jewish children's attainments were of a higher standard in languages, mathematics, chemistry, physics, shorthand and mathematical geometry. In natural history, geography, drawing, punctuality, diligence and behaviour they were also superior. The author believes in "eine unleugbar stärkere intellectuelle Veranlagung der jüdischen Schüler," but leaves open the question whether this superiority is due to earlier maturity among them or to an innate superior function. Contrary results were found by Jacobs(2) in 1922 who, examining the reports of 10-year-old children in reading, writing, spelling, arithmetic, geography and history, concluded that the Jewish boys are on a slightly lower level and are more variable in their attainments than non-Jewish children.

In New York in 1920 Miss Murdoch(3) endeavoured to throw light on this problem by the application of intelligence tests. She tested 489 Jewish children, 485 American and 491 Italian children, chiefly of the ages 10, 11, 12 and 13. The Jewish children were found to be on approximately the same level as the American children, and far superior to the Italians and the Negroes, the latter doing better in the tests than the Italian children. In the three largest age groups, 10, 11 and 12, the average was 15·5 per cent. for Italians, 30·5 per cent. for the Negroes, and 53·7 per cent. for the Americans, equalizing or exceeding the median for the Jewish children. The tests, however, involved the use of verbal concepts and introduced a language handicap chiefly felt, it seems, by the Italian children, and which makes the appraisal of the results difficult.

Pintner and Keller(4), in 1922, using a revision of the Binet test, examined school children in Youngstown, Ohio. The average I.Q. for the seventy-nine Jewish children, irrespective of whether they spoke English or a foreign language, was 95. The following table gives the scores of the English- and foreign-speaking groups:

English-speaking groups			Foreign-speaking groups		
No. of cases	Nationality	Average I.Q.	No. of cases	Nationality	Average I.Q.
249	American whites	95	313	Italian	84
71	American coloured	88	130	Slavic	85
24	English	97	37	German	91
			18	Roumanian	97
			4	Polish	85
			10	Russian	89

The I.Q. for the total English-speaking groups is higher than that for the foreign-speaking groups.

	No. of cases	Average I.Q.	Median I.Q.
English-speaking	367	92	94
Foreign-speaking	674	84	85

In Performance tests, however, the scores of the foreign-speaking children were higher. The author writes: "In comparing results obtained from a group of cases, given the Stanford Revision of the Binet test and a series of Performance tests, we find the correlation between the tests considerably better for the English-speaking group than for the foreign-speaking group and there were 23 per cent. more cases of the foreign-speaking children than of English-speaking where the performance age was higher than the mental age." It can hardly be doubted that the language handicap partly accounts for the lower scores of the foreign-speaking groups.

It may be interesting in this connection to glance at Brigham's(5) analysis of the intelligence of American recruits during the War, which was published in 1923. Surprisingly he finds the intelligence of the Jewish adults to be below that of all other nationalities except Polish and Italian. He also observed great variability amongst them, which fact he thinks would explain the popular belief in the superior intelligence of the Jews. "An able Jew is popularly recognized not only because of his ability but because he is able and a Jew." Brigham, however, makes no allowance for the fact that many of the soldiers could not speak English well; and, when he finds that those who had been a longer time in the country do better in the tests than the more recently arrived immigrants, he adopts the fantastic assumption that the recent immigrants are innately of a lower intelligence. To explain the higher scores of the northern Negroes as compared with the southern Negroes, he assumes that, by a selective process, the more energetic and able Negroes emigrate to the north. Rather than accept this assumption for which no evidence exists, it seems more probable to suppose that the environmental *milieu* of the northern Negro is more conducive to intellectual development than

the environment of the south, and that the effect of this superior environment is reflected in the intelligence scores.

Feingold (6) in 1924 in a careful and detailed study found only slight differences between the American and foreign-born whites. Applying a modified form of the Army Alpha to 2353 high-school freshmen he concluded that:

(1) The mental differences between foreign children reared in an American environment and children of Anglo-Saxons are too small to be significant.

(2) Jewish children of high-school age have almost the same mental rank as American children in their freshman year, and are only a few points lower in their junior year.

(3) Increasing differences in social opportunities after the sixteenth year account for the increasing differences in the scores between the high-ranking and low-ranking racial groups.

In this study the language handicap did not exist as all the high-school freshmen would speak English well. The Jewish pupil's slight inferiority in his junior year can be explained, as Feingold points out, by reason of the environmental check to his development as he grows older. He meets with a more restricted field of development than does the child of Anglo-Saxon parents. Feingold also observed that, whatever the social standing of the Jewish child's parents, every endeavour is made to send the child to a high school and give him encouraging stimuli to do well in school. It is impossible to overrate the importance of these factors in any study of the intelligence of the Jewish child.

Another important factor in this connection is the supposed earlier maturity of the Jewish child: W. F. Dearborn (7) stresses this point. In an investigation which he conducted in 1924 he examined almost equal number of Jewish, Italian and American children living in the same district. The Jewish children ranked highest both in mental age and intelligence quotients. They also, however, ranked first in anatomical development, as indicated by the stage of ossification of the carpal bones and expressed in terms of anatomical ratios. He maintains that: "The Jewish superiority and intelligence is partly due to greater anatomical and probably correspondingly greater physiological development, in that the slower growing American group will either develop for a longer period, or that their growth will be more accentuated during its later stages. So that at the completion of the period of growth, the critical disparity between the intelligence of the two groups would disappear. This result would depend upon the duration of the period of growth and the amount

of later acceleration. If both factors were operative the American group may in the end excel in intelligence." Much further research, however, is needed before these points can be determined.

A. Fukuda (8) in 1925 found the median I.Q. of Jewish children to be 82, which was much lower than the score of the English-speaking groups and lower than the scores of the foreign-speaking groups except Italian children. He had, moreover, only three Jewish children in his sample. The following are the scores of the English- and foreign-speaking groups.

	No. of cases	Median I.Q.	Average I.Q.
Total English speaking	109	96	95
Total non-English speaking	148	92	91

V. Graham (9) in a study of children at habit clinics applied the Stanford test and found that the Jewish child came out best. Below are given the scores of the different groups:

Nationality	No.	Median I.Q.
Jewish	47	105
American	76	99
Italian	43	85
All nationalities and others	167	95

In this investigation the least variability was exhibited by the Italian children.

In 1925 Karl Pearson (10) published the results of a survey which he commenced in 1913 into the problem of alien immigration into England. His results show that not only physically but mentally the Jewish children were inferior to the non-Jewish children. He analysed teachers' estimates respecting 619 Jewish boys and 585 Jewish girls, and found that "Jewish girls have less intelligence than the Gentile girls in any type of Council School. The comparison of Gentile and Jewish boys is less clear cut.... They (the Jews) are not as good as the boys of the medium or average school, but are better than the boys of the poor type of school. What is definitely clear, however, is that our alien Jewish boys do not form from the standpoint of intelligence a group markedly superior to the natives. But that is the sole condition under which we are prepared to admit that immigration should be allowed." How far this desire, so abruptly introduced in the last sentence quoted, coloured the investigation it is difficult to say. A later investigation carried on by Davies and Hughes (16), who applied standardized intelligence tests, arrived at conclusions completely opposed to those of Pearson. The following tables are taken from Pearson's survey:

Frequency distribution of intelligence—alien Jews.

	Very able	Capable	Intelligent	Slow	Dull	Very dull	Mentally defective
Jewish boys (%)	3·1	12·9	36·2	34·7	11·0	1·9	0·2
Jewish girls (%)	1·2	6·3	25·5	37·8	18·1	10·4	0·7

Intelligence distribution in twelve London schools of native Gentiles.

	Particularly clever	Above average	Average	Below average	Very dull
Gentile boys (%)	10	25	45	17	3
Gentile girls (%)	8	19	53	16	4

In criticism of Pearson's results it may be pointed out:

(*a*) Pearson's data as regards the intelligence of Jewish children consisted of teachers' estimates, which are very unreliable.

(*b*) The ratings for the Jewish and Gentile children were made by different teachers.

(*c*) Nearly one-third of the children (28·3 per cent.) were born abroad and not all knew the English language well. Only 6 per cent. of the fathers were born in England or naturalized, while 94 per cent. were foreign-born, and in these homes English would not be spoken.

(*d*) 71·9 per cent. of the fathers were engaged in occupations of low-grade labour. And although, as Pearson points out, the occupation of the father is no criterion of the innate intelligence of the child, it certainly influences its manifestation. A number of investigations have shown that the children of the economically more comfortable classes rank higher in intelligence tests than those who are lower in the social scale.

(*e*) The Jewish children surprisingly were found to contain a larger number badly nourished than the Gentile children. This must have had some effect on their school work.

(*f*) Pearson's main thesis that no correlation exists between intelligence on the one hand, and physique, health, care of the parents, economic and sanitary conditions of the home, on the other, is not yet conclusively established.

An American investigator, Miss Luckey (11), working on the same problem in 1925 in Cleveland, found that the Jewish children ranked only a few points below the American children and were second to them. The rank order of the groups was as follows:

(1) American children.
(2) Jewish ,,
(3) Hungarian ,,
(4) Bohemian ,,
(5) Coloured children.
(6) Polish ,,
(7) Italian ,,

In a second survey she examined 14,000 children who had been through a clinic, which generally received those who were above and below normal. The majority of the children were between 5 and 8 years though the range was from 4 to 21 years, and both sexes were lumped together. No national or racial group had less than 200 representatives; the Terman intelligence tests were used. Although the Jewish group had only as many as were examined in the Polish group, it had over forty times as many children who could be called bright: it had, on the other hand, less than a quarter as many children who were so seriously retarded that the regular school work could not be given them. The Jewish group had the largest percentage of bright children and the smallest percentage of retarded, as the following figures show:

Percentage of children classed as borderline or below (I.Q. below 80)		Percentage of bright children (I.Q. above 110)	
Negroes	65	Jewish	24
Polish	65	American	22
Slavish	63	German	8
Slovenian	58	Bohemian	6
Italian	55	Slovenian	3
Hungarian	47	Hungarian	2
German	46	Slavish	2
Bohemian	41	Polish	1
American	30	Negro	1
Jewish	29	Italian	½

As regards children classed as 'Imbeciles,' the interesting fact emerged that the Jewish and American groups had as many as any other group, in fact, more than the Negro and Slovenian groups.

A preponderance of gifted Jewish children was also noticed by Terman (12). Although the proportion of Jews in the three main cities from which the gifted children were selected was 5 per cent., more than 10 per cent. of the children were of Jewish blood.

In 1926 Miss Bere (13) made a study of 10-year-old boys of foreign parentage in New York, by means of the Stanford-Binet, Pintner-Patterson performance, National, and Pintner non-language tests. On the Stanford-Binet, Pintner non-language and the National tests, the boys ranked:

(1) Jews. (2) Bohemians. (3) Italians.

On the Pintner-Patterson performance tests, however, they ranked:

(1) Bohemians. (2) Italians. (3) Jews.

A similar study by N. D. M. Hirsch (14) who tested 5504 children in schools from Grade I–IX inclusive, chiefly by the Pintner-Cunningham

primary tests, revealed that the Jewish children scored best. The rank order according to the scores was as follows:

(1) Polish Jews.	(6) Americans.	(11) Poles.
(2) Swedes.	(7) Lithuanians.	(12) Greeks.
(3) English.	(8) Irish.	(13) Italians.
(4) Russian Jews.	(9) British Canadians.	(14) Negroes.
(5) Germans.	(10) Russians.	(15) Portuguese.

The author believes that the differences in intelligence are national or natio-racial, rather than social. It is difficult to see, if this is the case, why Polish Jews rank higher than the Russian Jews. Racially, both groups are very much alike; it is more than probable that environmental differences account for the superiority of one over the other. The author further believes that the language factor is negligible, and that intelligence is related to occupation as cause is to effect. Both of these assumptions have not the slightest evidence to support them.

Miss F. L. Goodenough (15) also believes that racial differences in intelligence have been established. She is of the opinion that the foreign groups are not handicapped by the language difficulty, since, in performance tests, equal ratings do not follow, ignoring the other and more pervasive influences affecting the result. She also maintains that an inferior environment is an effect, as much as it is a cause, of inferior ability, as indicated by intelligence tests. Dealing with large numbers of individuals as distinct from a defective line, which may sink into a low environment because of inferior intelligence, there is nothing to justify her beliefs, and even with such lines, taking the Jukes and Kallikaks, we do not know how many would be decent citizens had they been blessed with a better environment. After examining 2457 American-born young children in California, from various stocks, by the Goodenough tests, which consist chiefly of drawing exercises, and which eliminate any language difficulty, Miss Goodenough comes to the conclusion that superiority in intelligence lies with the Jewish children, whose median I.Q. was 106·3. She found that the coefficient of variability is least for the Jewish group indicating, she maintains, either the effects of selective immigration or ethnological purity. Brigham (5), on the other hand, as we have shown above, credits them with a high variability. The results she obtained are given in the following table.

The author claims validity for her results in that they correspond closely to the rank order of tests which involved the use of language. This, however, is no argument for accepting the results, since agreement between

different tests proves nothing, especially as the tests used different concepts and were measuring rods with no fixed units for measurement.

Stock	No.	Median I.Q.
American	500	100·3
Armenian	123	91·8
Italian	456	87·5
Spanish Mexican	367	87·2
Californian Negroes	69	82·7
Southern Negroes	613	76·5
Hoopa Valley Indians ...	79	85·6
Jewish	55	106·3
Chinese	25	103·1
Japanese	42	99·5
Germans	29	98·8
Portuguese	11	93·9
English and Scotch	14	99·5
French and Swiss	14	97·8
Danes, Swedes, Norwegians ...	31	104·5
Assyrians, Slavonian, Serbians	29	94·5

One of the most thorough investigations into the intelligence of Jewish and non-Jewish children was that of Miss M. Davies and Mr A. G. Hughes (16) in 1927. Nearly 2000 children in London from 8 to 14 years of age were given the Northumberland standardized tests (1925 series) which consisted of exercises in general intelligence, problems in arithmetic, and English, reading, spelling, history, geography and composition. The results were checked by the ratings of teachers who were in complete ignorance of the test results. In order to eliminate differences in environment, three London schools were chosen, each representative of a different social *milieu*, and each possessing approximately equal numbers of Jewish and Gentile children. The following are the authors' conclusions.

1. On an average, both in general intelligence and in attainments in English and arithmetic, Jewish children are definitely superior to the non-Jewish children attending the same school, the superiority being more marked with the boys than with the girls. This superiority is found at every age from 8 to 13 and in every type of elementary school tested. The superiority in intelligence revealed by the tests is confirmed by the independent estimates of the teachers.

2. The racial difference in favour of Jewish children amounts in the case of boys to about one year in intelligence and in arithmetic, and to a year and a half in English at age 10. Among girls the differences are equivalent to a year in English and to slightly less than a year (0·9) in intelligence and arithmetic.

3. Variations in the differences between Jewish and non-Jewish children are found at different ages, the noticeable and consistent being a temporary drop at age 12 followed by a distinct rise at age 13. There is no indication in the data (which concerns children from 8 to 13 years of age) that the Jewish children's precocious development is followed by a premature arrest.

4. No sex difference in English and arithmetic is found among non-Jewish children: Jewish boys are on the average superior in these subjects to Jewish girls (the equivalent of about four months' progress at 10 years of age).

In intelligence the Jewish boys are slightly superior to Jewish girls, but the contrary result is found among non-Jewish children.

However ably the investigation was carried out, it is doubtful whether the environment in the widest sense of the word was the same for both groups of children. "The choice of schools," the authors write, "was designed to rule out differences due to variations in school training and in home training (except in so far as the latter are due to racial customs and traditions)." In "racial customs" and traditions we already find a difference in environment. Elsewhere Mr Hughes(17) shows that the Jewish children were better fed and clothed, that the economic position of the Jews is less static than that of Gentiles and that, even in poor districts, they may live on a higher scale than their neighbours; finally that the Jewish child may develop more quickly than the Gentile child. Nevertheless the author believes that the occupational and social differences between the two groups was not great, and his belief in the Jewish child's superiority is strengthened by an analysis of the intelligence of the Jewish and non-Jewish children in different occupations. He classifies the occupations of the parents into the following groups:

(1) Manufacturers, merchants, travellers, Rabbis, teachers, higher grade clerks;
(2) Engineers, electricians, cabinet makers, metal workers;
(3) Shopkeepers, dealers and small masters;
(4) Policemen, postmen, bus and taxi-drivers, conductors, soldiers, sailors, railway porters;
(5) Makers of boots, clothes, umbrellas, buttons, boxes, cigarettes;
(6) Bricklayers, carpenters, painters, plumbers;
(7) Shop assistants, hairdressers;
(8) Carmen, coalmen, caretakers;
(9) Labourers (mainly in docks), hawkers;
(10) No father;
(11) No occupation;

and finds the differences between the children in similar occupational groups to be:

Group	1	2	3	4	5	6	7	8	9	10	11
Jews	109·7	107·7	110·1	105·4	103·9	105·7	102·0	112·4	103·8	100·6	85·8
Non-Jews	103·5	101·3	98·4	101·5	97·3	93·7	88·7	92·8	90·1	93·0	90·7
Difference	6·2	6·4	11·7	3·9	6·6	11·4	13·3	19·6	13·7	7·6	4·9

In Groups 4, 6, 8, 9 and 11 there were less than twenty children.

From an analysis of this table the author concludes that, in Groups 1, 2 and 5 the differences on the average was 6·5 or half a year at the age of 10, and that in Groups 3 and 7, since the differences were twice as great,

the more brilliant Jewish children are indicated, commerce-attracting, so his argument runs, Jews of very good intelligence.

What, however, seems to be more significant, if we include Groups 4, 6, 8 and 11, is that the difference between the Jews and non-Jews increases as we go from the higher occupational groups to the lower. This is what we should expect if we compared the environment of Jewish and non-Jewish children whose fathers were merchants, and whose fathers were carmen. The difference in the latter case would be much greater, as could be testified by anyone who is acquainted with the social conditions of Jewish and non-Jewish working men. Whereas the difference in Group 1 is 6·2, in Group 8 it is 19·6. The home conditions of an English or Jewish coalman or caretaker would be worlds apart. The stimulus and encouragement to study and the sobriety of the wage-earner, which are found in the lowest of Jewish homes and which are a part of the Jewish tradition, are not found as frequently in similarly situated English homes. The assertion of the author that the best type of Jew goes into commerce is an unproved statement, and certainly he has not proved that "after environment is eliminated there still remains a racial difference equivalent at the age of 10 to about 0·7." Environment has not been eliminated, and it is this factor which probably accounts for the superiority of the Jewish children.

Miss Irma Loeb Cohen (18), in her thesis on the comparative intelligence of the Jews, also found a small but consistent superiority of the Jewish over the non-Jewish students at Ohio University.

Seago and Koldin (19) examined sixth grade pupils of Jewish and Italian nationality. Although no language handicap was involved the Jewish students scored higher than the Italians.

Prof. E. C. Hayes (20) reviewed the scholastic records of some racial groups in the University of Illinois and obtained the following rank order:

(1) Chinese.
(2) Jews.
(3) Nordics.
(4) Foreign students (excluding Chinese).
(5) Alpine.
(6) Mediterranean.
(7) Negroes.

The average record, however, of each racial group was below that of the general mixed group.

The language handicap was investigated by M. Rigg (21) in 1928. He tested Grades III–VIII in elementary schools, in the National intelligence tests which partly involves the use of language, in the Woody-McCall arithmetic test which hardly involves language, and the Thorndike-

McCall reading test which is essentially a language test. A glance through the subjoined table shows that a definite language handicap existed among the foreign groups:

Group	No.	Intelligence quotients		Arithmetic quotients		Reading quotients	
		Median	P.E.	Median	P.E.	Median	P.E.
Native	8130	104·85	0·17	102·96	0·15	100·28	0·17
Foreign	1949	103·30	0·34	104·26	0·31	98·06	0·35
German	1095	104·69	0·44	104·64	0·39	99·61	0·45
Jewish	445	103·19	0·82	106·10	0·72	98·35	0·76
Italian	140	91·43	1·13	93·81	1·28	86·74	1·11
Bohemian	118	104·00	1·22	104·56	1·28	96·67	1·39
Miscellaneous	151	105·14	1·08	104·33	1·04	98·47	1·18

In spite of a reliable difference in favour of the native group on the intelligence tests, there is a reliable difference in favour of the foreign group in the arithmetic test. In the reading test, which was largely a language test, the native group did best. Although every group did not score as well on the reading test as on the arithmetic test, the foreign children fell short to a much greater extent than the native, as an analysis of the drop between the median arithmetic quotient and the median reading quotient, for the various groups, shows:

Native	2·68	Italian	7·07
Foreign	6·20	Bohemian	7·89
German	5·03	Miscellaneous	5·86
Jewish	7·75		

According to the author the language handicap by itself is sufficient to explain the lower standing of most of the foreign groups on the intelligence tests. The German Jews and Bohemians, it is to be noticed, are on about the same level, whilst the low rank of the Italians cannot be accounted for, writes the author.

Several surveys have shown that the language handicap is of great importance in determining the results of intelligence tests. G. L. Brown (22) has shown that immigrant children rated 18 months' higher when tested in their own language. Mead (23), testing Italian children in America, found that the mean score in the tests is positively correlated with the amount of English spoken in their homes, with the period of time their fathers lived in America, and with their progress up the higher grades in the school. Walters (24) came to the conclusion that there is a language handicap in the Stanford Revision of the Binet-Simon tests of 6 to 8 months' mental age for foreign children of 13 years. Flores (25) studied English and Spanish children in the same school, and found that

there were significant individual differences in the degree to which the language difficulty is experienced among the Spanish-speaking children. Everywhere children have ranked higher in tests not involving the use of verbal concepts than in those where such concepts were employed.

Of the nineteen investigations listed above, eight maintained that Jewish children were superior to non-Jewish children, whether in England, Germany or America, four that they were inferior, and four that no appreciable differences exist; in two of the investigations the Jewish children compared with Italians and Bohemians were found to be superior, and in one, where scholastic excellence was the criterion, the Chinese were first and the Jews second. In some instances the superiority or inferiority was of such a slight character that it becomes doubtful whether it is permissible to use such descriptive terms at all. Even if differences in intelligence do exist between the various national or racial groups, its expression, as measured by the tests, was so overlaid by so many other factors that it becomes almost certain that we are not dealing with any innate differences. And if this be the case there is no cause for checking the immigration into England or America on biological grounds of 'inferior' East European peoples. As regards the Jews the consensus of opinion that they are superior in intelligence may be correct, but this superiority has not been proved to be hereditary, and probably has its roots in the traditions, education and general superiority of the Jewish environment.

The claims that were made for intelligence, when they first spread through the educational world, have not been substantiated. The reaction to the view that they are an innate measure of ability is very pronounced to-day. For even if we are agreed upon a satisfactory definition of intelligence—and there is more agreement as to who is intelligent than what intelligence is—the impossibility of evaluating the importance of environmental factors, which alter the expression of intelligence, makes the tests almost worthless in value for ordinary school purposes. Their sphere of greatest utility lies, in the present at any rate, in the field of somewhat subnormal intelligence. As tests of retardation in intelligence and school development they are of great value. Intelligence tests have certainly not measured what intelligence is as defined in the definitions given below:

(1) Intelligence is judgment or common sense, initiative, the ability to adapt oneself. (Binet.)

(2) Intelligence in general is the power of good responses from the point of view of truth or fact. (Thorndike.)

(3) Voluntary attention is the essential factor of general intelligence. (Burt.)
(4) Intelligence is intellect plus knowledge. (Hemmon.)
(5) Intelligence is an acquiring capacity. (Woodrow.)
(6) Intelligence is the ability to think in terms of abstract ideas. (Terman.)
(7) Intelligence is the relative general efficiency of minds measured under similar conditions of knowledge, interest and habituation. (Ballard.)

Nor have the tests been applied "under similar conditions of knowledge, interest, and habituation." For a just evaluation of intelligence tests, the following conditions are of the utmost importance:

1. The social status of the persons examined and their occupational alignment. Differences in these will result in differences in performing the tests.

2. The age data and variations in age or grade in school should be known precisely.

3. Any language handicap must be examined. As Boas(26) points out: "In all tests based on language, the effect of the linguistic experience of the subject plays an important part." "Comparison of reactions of individuals that speak fundamentally distinct languages, may therefore express the influence of language upon the current of thought, not any innate difference in the form of thought." Every group it must be remembered has had an historical and social background peculiar to itself and consequently possesses different modes and patterns of thought. We have shown above the effect of language handicap on a number of tests.

4. How many of those examined are foreigners, and the duration of time they have lived in the new country should be known.

5. The variability in the group examined and the amount of overlapping among the different groups should also be known.

6. More needs to be known about the relationship between physical and mental growth. Dearborn(7), as we saw above, thinks that the superior intelligence of the Jewish child is due to his more rapid anatomical and physiological development. Miss Wentworth in 1926(27) found a definite relationship between mental and anatomical development. In about two-thirds of her cases retardation in anatomical development accompanied mental retardation, and acceleration in anatomical development accompanied mental superiority. It may well be that the superiority of one year in mental age which the children of the upper classes exhibit is simply an indication of their earlier maturity.

Cozens in 1927 (28) in a similar study showed that:

1. The better developed a boy or a girl is for his or her age the more able are they in school work.
2. The general level of the physical development of children who rate high on intelligence tests is superior to that of children who are low on intelligence tests.
3. There seems to be a direct relationship between ability and physical tests and promotion or scholastic results.

Freeman and Carter (29) and Abernethy (30), on the other hand, studying such items as dentition, age of maturity, height, weight, and the ossification ratio of the carpal bones, and correlating these with results on the Stanford-Binet tests, found that children who are precocious in physiological development are not necessarily of high intelligence.

As regards the test itself, it is as yet uncertain whether the I.Q. itself remains constant as the children tested grow older or are transposed to a new environment. Freeman (31) found an increase of from 7 to 10 points in the I.Q. of foster children placed in a good environment, and Burke found an increase of from 3·9 points (32). It is now realized that a single I.Q., whether based upon an individual or a group test, is unreliable. Symonds (33) lists twenty-five factors that can effect the reliability of a test as, for instance:

(*a*) the construction of words in a test;
(*b*) the speed of taking a test;
(*c*) accuracy of a test;
(*d*) the incentive or effort put in;
(*e*) the obtrusion of competing ideas;
(*f*) distraction;
(*g*) accidents occurring during the examination;
(*h*) illness, worry or excitement;
(*i*) the more common the experiences called for in a test are to the members of the group taking the test, the more reliable it is;
(*j*) the more homogeneous the material of a test the greater its reliability;
(*k*) chance in getting a correct answer to an item in the test is a factor in test reliability;
(*l*) the longer the time a test occupies the more reliable it is.

All these and the personality of the examiner, the subjective and objective elements entering in, the variability in memory and speed of the children, their habits of attention and comprehension, the effect of

practice and change in physical condition, will affect the results of a test. It is no wonder, therefore, that competent observers decry the value of intelligence tests. Thus H. N. Bond(34) writes: "The consensus of competent scientific thought, contemplating the inability of mental testers to define intelligence, the inadequacy of all attempts to take such factors as education, social status and language into proper consideration, and the deficiencies of testing conditions, finds no proof of racial inferiority or superiority and eliminates the usual methods of determining such standing from the field of scientific usefulness." Prof. H. C. Link(35) says: "Nothing in the technique of intelligence tests as applied so far, warrants any comparison whatsoever between the intelligence of various groups and races." The opinion of Franz Boas(26) is of special importance. "The test itself only shows that a task set to a person can be performed by him more or less satisfactorily. That the result is solely or primarily a result of organically determined intelligence is an assumption that has to be proved. Since all functions are strongly influenced, by environment, it is likely that here also environmental influences may prevail and obscure the structurally determined part of the reaction." "The responses to tests may be based on recognition of sensory impressions, on motor experience such as the result of complex movements, or on the use of acquired knowledge. All of these contain experience." "A critical examination of all studies of this type in which differences between racial groups in regard to mental reactions are demonstrated, leaves us in doubt whether the determining factor is cultural experience or racial descent. For large racial groups acceptable proof of marked mental differences due to organic not social causes has never been given." The present tendency is to pay more attention to the subtle influence of environmental factors. Even among twins where the influence of heredity was supposed to preponderate it is now seen "that their resemblances are never all exclusive and differ in delicate ways, just as their finger prints may be of the same pattern and differ in the finer measurements(27)." Miss Weill(36) shows in her monograph how many are the varying factors that can enter into the determination of the behaviour of the children of the same family, so that each one's behaviour is different. Even in such remote factors as the quality and condition of the distant and immediate ancestors of an individual, not only germ plasm but social heredity and the influence of tradition are involved. The lasting importance of pre-natal and post-natal conditions are being more and more recognized by doctors and educationalists.

Intelligence tests to have more value than they do now must certainly

be improved and made applicable not only to children but also to adults. Furthermore, valid methods for measuring all phases of human behaviour must be devised. To learn anything of the psychological make-up even of a child, we need besides a series of intelligence tests an individual study of the child from all angles, physical, social and emotional—for all these will affect the expression of intelligence. "A low I.Q. does not necessarily prophesy failure, nor a high I.Q. success. Wrong habit reactions formed early in childhood, or in the first years of school life, are at the basis of many neurotic symptoms of much that passes for mental deficiency. This is especially notable in poor habits of attention, memory, and learning (27)."

Besides intellectual differences between Jews and non-Jews, temperamental and emotional differences also are supposed to exist. This view seems very plausible, but little experimental work exists to enable us to speak with any certainty on the matter. Kretschmer (37) is of the opinion that the Jews possess specific types of temperament which are correlated with the high frequency among them of tuberculosis, diabetes, heart disease and arthritis; their distinct physical constitution depends, he believes, on a distinct glandular balance which carries with it mental qualities of a particular kind. Nemeck (1), in his study mentioned above, believes that the Jewish Ashkenazi children are livelier and more emotional than non-Jewish children; the Jewish Sephardi children, on the other hand, are placid and oriental. He is of the opinion, too, that Jewish children think verbally and non-Jewish children pictorially, and that the mental processes of the former proceed in a quicker tempo. In an article in the *Journal of Comparative Psychology* in 1923 (38), it is maintained that Jewish women are more disposed to intellectual pursuits than South Italian women, who take to church-going, and French girls who take to theatre-going and a life of pleasure.

Leydesdorff (39), in a survey of the emotional differences between Jews in Holland and non-Jews, using Heyman's and Wiersma's valuations, found that Jews are much more emotional than Gentiles. The following are some of his results:

	Jews	Non-Jews
Emotionally inclined (%)	63·8	52·6
Quickly offended (%)	52·9	43·5
Good natured (%)	38·6	52·6
Of a critical temperament (%)	53·4	38·4
Anxious and suspicious (%)	41·0	30·9

Jews are also less phlegmatic, but more nervous, sentimental and

passionate. They are not as musically inclined as non-Jews, but they are more intellectual. They are fond of good eating and drinking.

	Jews	Non-Jews
Viel auf Essen und Trunken halten (%)	55·9	38·8
Regelmässig Alkohol geniessen (%)	1·9	12·5

Hartshorne and May (40) enquired into the comparative deceitfulness of different national groups, and its relationship to intelligence. They found that the Jewish children from a good environment appear more deceptive than is expected from their level of intelligence; when, however, all class-room cheating tests are combined, Jewish children tend to cheat less than any other race. Jewish children who come from broken homes cheat once in every two opportunities.

Emotional and temperamental tests, being as yet in their infancy and unstandardized, cannot have much value. This also applies to the judgments of psychologists and sociologists on the racial characteristics of the Jewish people. They are of too subjective and contradictory a nature to possess scientific value, although they are of interest in revealing popular beliefs and their origin and crystallization in tradition. Crew (41), the biologist, writes: "They are clever and intuitive, have no capacity to form a political state, but flourish as minorities among other peoples. Inclined to abstraction, logic and dialectics, highly musical, eloquent and monotheistic." Schwalbe and Fischer (42) maintain that the Jew is characterized "in geistiger Beziehung starker Wille, grosse Intelligenz, Lebhaftigkeit und Unternehmungslust, gute Fähigkeiten beim wirtschaftlichen Tauschakt, überhaupt grosse wirtschaftliche Begabung. Die genialen Höchstleistungen des Nordeuropäischen Typus werden allerdings nur selten erreicht." Stoltheim (43), an anti-semitic writer, accuses the Jew of being "sharp at baseness, glib of tongue, greedy for money and of a saving disposition, cunning, addicted to dissimulation, averse to bodily labour, sensual, shameless, vain, cowardly and impudent." And McDougall (44), forgetting his scientific calling as a psychologist, fantastically argues that psycho-analysis "which was evolved by a Jew, who studied chiefly Jewish patients" and which appeals very strongly to Jews, "may be approximately true of the Jewish race." Von Wiessen (45) claims that whilst the Germans are instinctive, plain, kind, healthy, phlegmatic, musical and moral, the Jews are characterized by "Beweglichkeit des Geistes, eine wache Interessiertheit, einen raschen Blick für die Situation, geschickte Beweglichkeit und Vielseitigkeit." Leydesdorff (39) writes: "Ferner überwiegt

bei den Holländischen Juden, Ehrgeiz, Geldsucht, Flottheit in Geldangelegenheiten, Herrschsucht, Mitleid, Hilfsbereitschaft, philantropische Tätigkeit, Natürlichkeit im Auftreten, Neigung zum Übertreiben, Ausschmücken,...Furchtsamkeit."

It is interesting to notice that some of the above traits, whether true or not, are *ex post facto* deductions of the Jew *par excellence* as an urban dweller. It is hardly remarkable that a nation for so many centuries commercialized should possess some of these attributes, but that they are innately racial is absurd. An historical study of the opinions that have been passed on the Jew would discover a remarkable sameness and continuity to some extent of these judgments, for tradition having once created a typical and by no means favourable Jew, has passed him down the centuries with little change. Prejudice rather than scientific acumen or dispassionate analysis is responsible for the judgments we make upon members of alien groups.

As regards a number of traits there is no agreement as to whether they are or not characteristic of the Jewish people. Contradictory judgments are frequent, and whilst one psychologist writes that the Jew is distinguished by a passion for abstract thought, another maintains precisely the opposite view. Thus we hear from different psychologists that the Jew is musical and non-musical, sensual and ascetic, sentimental and non-sentimental.

What becomes evident after the review above is that work of value as regards the measurement of racial mental characters and of behaviour which are determined through racially inherited nervous mechanisms does not yet exist. The approach to such knowledge through scientific studies of language is also necessary. Yiddish and Hebrew are in this respect uncharted fields.

Furthermore, the high incidence of certain kinds of nervous diseases among Jews is not racially determined nor correlated with any anatomical or physiological peculiarities in their nervous system. Prevalent functional diseases, such as neurasthenia, hysteria and melancholia are urban diseases, common to urban communities, daily consumed by ambition and anxiety. The long Jewish history of exile and persecution is another cause for the frequency of these diseases. "The Ghetto life was not only unwholesome physically, but unwholesome mentally, emotionally and spiritually. Living in constant dread of massacre, exposed to ridicule, degradation and more sinister disasters, the race developed apprehensiveness and acquired a lower threshold for fear stimuli. This was kept up by the drawing in towards an over-intimate family life (46)." Further-

more, it is to be expected that survivors and spectators of pogroms should have their nervous system shattered.

Biological factors, as distinct from environmental factors which we have stressed hitherto, may have been of more importance in determining the number of mental defectives among the Jews. Living in concentrated and exclusive communities, the Jews frequently inbred, and thus latent defects would rise to the surface. Inbreeding and selection are supposed to explain the excessive number of exceptionally capable and gifted Jews (see Jacobs(47) and Bienstock(48)). Gifted strains frequently married, for a studious son-in-law was preferred to a rich one. We do not know, however, the extent of such inbreeding, nor whether the number of eminent Jews is proportionately larger than in other communities. It is also maintained that the ages of persecution were instrumental in weeding out those who were mentally too feeble to withstand the severe struggle, and leaving the most intelligent and subtle to perpetuate their kind. Hankins(49) writes of the Jews that "their adaptability and intense concentration may be largely racial, having been accentuated by inbreeding and selection, which were favoured by persecution and exclusiveness. These traits are not peculiar to Jews only, though they may have them in greater frequency. Their financial acumen is likewise based on biological and social inheritance." On the other hand, it is equally possible to argue that the finest types of men were eliminated in the persecution.

The unscientific assumption of racial superiority or inferiority has hindered the advancement of racial psychology. An exact technique which will eliminate or make allowance for nurture, and which will secure really random samplings, and evaluate the whole historical environment of a group, will have to be devised before we can range the various races in a hierarchy. As it is the little knowledge that we have acquired on the subject of racial psychology makes once and for all impossible the resuscitation of the Nordic myth, which assumed that such a hierarchy based on innate physical and mental differences exists. History shows us that the achievement of the different nations and groups is no indication of differences in biological worth, and the reaction against the view which would explain complex social phenomena in terms of biology is certainly to be welcomed.

Intelligence tests on Jewish children.

Year	Authority	No. of Jewish children examined	Age (years) or grade	Test	Results
1916	Nemeck	555	High school 15–18	School subjects	Jewish children superior to German
1922	Jacobs	—	10	School subjects	Jewish children slightly inferior to German
1920	Murdoch	489	10–13	Pressey	Jewish boys equal to American whites and superior to Italians and Negroes
1922	Pintner	79	—	Stanford-Binet	Average for all Jewish children speaking English or not was I.Q. 95. Equal to American whites and superior to others
1923	Brigham	Adults	—	Alpha & Beta	Average intelligence below all samples except Poland and Italy; Jews very variable
1924	Feingold	862	High school	Army Alpha modified	Jewish children of high school age have same mental rank as American children in freshman year and are few points below in junior year
1925	Dearborn	—	—	—	Jewish children superior to all groups due to more rapid anatomical development
1925	Fukuda	3	—	—	Median I.Q. of Jews 82 lower than all foreign-speaking groups. Much lower than English group
1925	Graham	47	—	Stanford	Median I.Q. of Jewish children 105. Superior to American (99) and Italian (85). Superior both in verbal and non-verbal tests
1925	Pearson	1204	—	Teachers' estimates	Jewish girls inferior to girls in any type of school. Jewish boys inferior to medium and good school but not to poor school
1925	Luckey	Over 200	Majority 5–8	Terman	Jews had largest number of brightest children and least retarded. American next
1926	Hirsch	—	Grades I–XI (incl.)	Piner-Cunningham primary	Rank: (1) Polish Jews; (2) English; (3) Russian Jews; (4) Germans; (5) Americans; (6) Lithuanians, etc.
1926	Bere	—	10	Stanford-Binet. Pintner non-language. National. Pintner-Patterson	In verbal tests: (1) Jews; (2) Bohemians; (3) Italians. In performance tests: (1) Bohemians; (2) Italians; (3) Jews
1926	Goodenough	55	—	Goodenough drawing	Median I.Q. for Jewish children 106·3 highest: low variability
1927	Hughes & Davies	1894	8–14	Northumberland standardized	General superiority of Jewish boys and girls in intelligence and attainments at about every age from 8–13 in every type of school
1928	Seago & Koldin	—	—	—	Jews superior to Italians: not due to language handicap
1928	Hayes	93	University students	Academic record	(1) Chinese; (2) Jews; (3) Nordics
1928	Rigg	445	Grades III–VIII (incl.)	National intelligence test; Woody-McCall arithmetic; Thorndike-McCall reading	Median I.Q. 103, slightly lower than native. Median I.Q. arithmetic 106; reading 98: language handicap involved

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A FURTHER NOTE ON EUPHASIA.

By T. H. PEAR.

(*From the Department of Psychology, the University of Manchester.*)

In this *Journal* (1930, xx, (4), 371) I suggested, with reasons, the term 'euphasia' to signify the ability for deliberate, adequate, verbal expression. The proposal has evoked two published comments.

Prof. C. E. Seashore, *Science*, lxxii, No. 1865, p. 318, acknowledges the need in psychology and education for such a term. Giving further reasons for making this distinction, he comments that 'euphasia' is unavoidably associated with terms of the same root in mental pathology, such as 'aphasia' and 'dysphasia.'

I doubt the unavoidability of the association. 'Eugenic' and 'dysgenic,' for example, are related in precisely the same manner that euphasic and dysphasic ought to be. Moreover, though for practical purposes it may be useful to separate mental pathology from psychology, I hold that theoretical psychology becomes sterile in doing so.

Prof. Seashore's kindly interest has caused him to seek for alternative words. He suggests 'euphany' and 'euphanious.' Yet I am advised by Prof. W. B. Anderson that they are unsuitable. "Although the active voice of phaino means 'show,' the middle voice means 'appear,' and it is the latter meaning that is found in the compounds mentioned above. 'Euphany' could only mean 'good appearance.'"

While, therefore, gratefully acknowledging Prof. Seashore's interest I feel compelled to support 'euphasia.'

In this *Journal* (1930, xxi, (2), 203) Dr H. D. Jennings White urges that this new term be not accepted, on the ground that 'verbal skill' covers all that is necessary. In proposing 'euphasia' I cited a previous article which, I hoped, would represent my reasons adequately ("Some Subtler Skills," this *Journal*, 1929, xx, (2), especially pp. 149 ff.). There it was suggested that anticipation, collection and selection should be regarded as the prelude to skill.

'Verbal skill,' skill with words, might thus be likened to skill with other instruments. A man may be simply skilled with instruments selected for him or he may anticipate, collect and select the instruments, before showing skill. Many reciters, actors and lawyers show verbal skill with words prescribed for them; Prof. William James was euphasic.

A term which "'covers' both vocal and scriptal forms of 'verbal expression'" may be useful. Yet in covering, it obscures the notorious differences between these two arts; expressed in the lines:

"He wrote like an angel, and talked like poor Poll."

Verbal skill marks off a limited, and in many quarters over-rated, ability. One popular movement in psychology suffers seriously from its development of low-grade verbal skill without euphasia.

PUBLICATIONS RECENTLY RECEIVED.

A History of Experimental Psychology. By Edwin G. Boring. New York; London: The Century Co. 1929. Pp. xvi + 699. $4.00.

As the title of this book indicates, the author is not so much concerned with philosophical psychology as with psychology as an experimental science. Hence he begins by tracing its antecedents in general and evolutionary science, in the physiology of the nervous system and special senses, in the personal equation of the astronomers, and in the popular cults of phrenology and mesmerism. The development of philosophical psychology is then dealt with. Descartes' doctrine of innate ideas was combated by the British empiricists; the greatest of these, Locke, was the founder of associationism, which played so large a part in British psychology, culminating with Mills and Bain. Nativism, developed in Germany by Leibnitz and Kant, was modified by Herbart and Lotze to a more scientific attitude which made possible the foundation of experimental psychology in Fechner's formulation of Weber's law. There followed the great days of 'sensationism' or 'content' psychology, during which the experimental work of Helmholtz and Wundt seemed to show that psychology would rapidly become an exact quantitative science. But the opposing 'act' psychologists, Brentano and his followers, stressed the importance of function rather than content. The former is much less open to introspection and experimentation than the latter. And though Külpe attempted to treat 'act' and 'content' as co-existent psychological material, it was clear that the problems of psychology could not be solved as readily by experimental methods as Wundt supposed.

With the exception of the school of Titchener, which chiefly developed the sensationism of Wundt, the tendency of American psychology, beginning from the empiricism of James, has been all towards functionalism, and the study of individual differences; hence the modern development of behaviourism, and of mental testing and applied psychology. A further revolt against the analytical methods of the sensationists has also been made in Germany by the 'Gestalt' psychologists. In this country, however, empirical philosophical psychology for long ruled the day, and experimental psychology, with the exception of Galton's anthropometric measurement, is of recent growth.

These various currents of psychological thought and experimental method and their mutual influence are traced by the author with admirable clarity, with a single exception. It is difficult to follow any general trend of thought in American psychology, or, indeed, to distinguish the wood for the trees. This may result from the fact that among the enormous bulk of American psychological work there are few outstanding figures, after James and Titchener. But it may be argued that this lack of clear cut tendency is all to the good. Too often such a tendency has arisen from a philosophical bias towards a theory which will cover the whole field of knowledge, often at the expense of suppression or distortion of experimental evidence. The author of this book points out how behaviourism has been weakened by the inclusion of the whole content of the older systematic psychology. Similarly the Gestalt psychologists have attempted to give their theories a generalized physico-chemical basis. It is the merit of this book that it shows how all these conflicting generalized theories have arisen and developed, and reacted upon and partially destroyed each other; hence the present position of psychology is made clear.

Minnesota Mechanical Ability Tests. By D. G. Paterson, R. M. Elliott, L. D. Anderson, H. A. Toops and Edna Heidbrede. University of Minnesota Press. 1930. Pp. xxii + 586. $5.00.

This book is the outcome of an "invitation from Dr Robert M. Yerkes, chairman of the National Research Council Committee on Human Migrations to Donald G.

Paterson and Richard M. Elliott...to submit proposals for research in the field of mechanical ability to be carried out at the University of Minnesota under a grant of financial support from the National Research Council." Dr Yerkes himself contributes a most enthusiastic foreword to the book, and beyond doubt his praise is thoroughly justified. Every person who is interested in methods of studying mechanical aptitudes, and particularly every student who wishes to attempt research in this field will find the volume a mine of information about tests, methods and established results. Moreover the information is generally expressed clearly and concisely. The volume ought to be in the library of every experimental psychological department in this country.

The Growth of Ability. By R. O. Filter and O. C. Held. Baltimore: Warwick & York. 1930. Pp. vii + 174. $2.28.

This is a somewhat untechnical general discussion of the learning problem. The authors favour a theory of the environmental determination of learning, assuming only such intra-organic causation as is required by a rather extreme Conditioned Reflex view. The first part of the book consists of a general critical discussion of the problem, mainly in terms of the results or views of other people. The second part presents some practical considerations about learning in an interesting manner. The final *resumé* sides with nurture against nature as the chief condition of learning, but the authors are by no means dogmatic. The volume is No. 28 of Educational Psychology Monographs.

The Psychology of the Common Branches. By W. H. Pyle. Baltimore: Warwick & York. 1930. Pp. vii + 381. $2.10.

The common branches are reading, spelling, handwriting and arithmetic, and this book attempts to set forth the fundamental principles involved in the teaching of these subjects. The exposition is uniformly clear, is accompanied by numerous illustrations, and each chapter is followed by a series of questions and exercises on the matter dealt with. Comprehensive bibliographies are given. The book, being in the main addressed to the working school teacher, is untechnical in style, but it is obviously based upon a great amount of careful experiment and on wide reading, and should most usefully fulfil the purpose for which it was written.

The Meaning of Sacrifice. By R. Money-Kyrle. London: Hogarth Press, Ltd. 1930. Pp. 273. International Psycho-Analytical Library, No. 16. 18*s.* net.

The volume consists of two parts, the first dealing with the Oedipus Complex and the second with the Meaning of Sacrifice. The author fully accepts psycho-analytic theories save that he considers it possible to achieve a satisfactory explanation of sacrifice without assuming any psychic continuity in the group or race, further than is secured by age overlap and persistence of institution and custom. Fundamentally sacrifice is based on the Oedipus complex and expresses a tendency towards parricide which may, however, be satisfied by a number of substitute objects. The essay is clearly written and carefully documented, and it expounds quite fairly a number of theories of sacrifice which are different from the one adopted. It is a contribution of value to psycho-analytical study.

Possession Demoniacal and Other; among Primitive Races, in Antiquity, the Middle Ages, and Modern Times. By T. K. Oesterreich. Translated by D. Ibberson. London: Kegan Paul, Trench, Trubner & Co. 1930. Pp. xi + 400. 21*s.* net.

The book is a mine of information concerning the phenomena of possession in all ages and amongst widely scattered peoples. Oesterreich maintains both that possession is largely a consequence of belief in spirits—often in malevolent spirits,—and also that above everything else in human history it has served to confirm and deepen this

belief. With advancing scientific enlightenment the phenomena become very much less common and the beliefs which are their basis and their consequence tend to disappear. More or less sporadic manifestations still appear but are to be considered as phenomena of 'regression.' The social psychologist will find in this book a mass of interesting and well presented data not overloaded with theoretical matter. The whole work displays wide learning, and a sanely balanced judgment.

Conversations with the Other World, Telepathy in This. Edited by ISOBEL GRANT. London: Williams & Norgate. 1930. Pp. 91. 2*s.* 6*d.* net.

These 'Conversations' are records of automatic writing experiments, the actual 'messages' being printed in italics. Unfortunately no information whatever is given as to the conditions of the experiment, and consequently, though the material may interest a good number of people, psychologically the book is of no value.

Twins; Heredity and Environment. By N. D. M. HIRSCH. Cambridge: Harvard University Press. (London: Humphrey Milford.) 1930. Pp. 159. 10*s.* net.

This is an interesting though not very extensive study of the physical and mental characteristics of twins. The literature of the subject is very well handled and such original observations as were made are clearly set forth. It is concluded that while both heredity and environment play important parts in determining human reaction, the significance of heredity is probably far greater in regard to mental than in regard to physical characteristics. This was particularly marked in relation to I.Q. differences.

The Mind of the School Child. By VALENTINE DAVIS. London: Cartwright & Rattray, Ltd. 1930. Pp. 343. 7*s.* 6*d.* net.

Mr Davis is, he says, "a teacher writing for teachers," and his book is to be judged by its contribution to the practice of teaching. Undoubtedly he has read widely, and has great enthusiasm for his work. Moreover the book contains a large number of lively stories, interesting suggestions and attractive exercises. But it is not well ordered and could reasonably have been considerably condensed. It would have added to the practical value of the work if some indication could have been given of sections that might well be omitted on a first reading. For example the first chapter, bravely entitled "What is Mind?" will probably merely confuse and 'put off' the average teacher. If the volume could be pruned, rearranged somewhat and made a lot simpler in places, it would be greatly improved. As it is it may still be of much help to the school teacher if it is read with discrimination and in a suitably critical frame of mind.

The New Generation; The Intimate Problems of Modern Parents and Children. Edited by V. F. CALVERTON and S. D. SCHMALHAUSEN. With an Introduction by BERTRAND RUSSELL. London: George Allen & Unwin, Ltd. 1930. Pp. 717. 20*s.* net.

This is a co-operative effort produced by thirty-two writers, of whom the majority are American. It is described as: "A handbook for parents and a Magna Charta for children." With one or two exceptions it is distinguished by the unnecessary length with which its authors explain their obvious disagreement with existing institutions and customs. Among the whole of the contributions one essay stands out as a brilliant, original and masterly production: that by Malinowski on "Parenthood—the Basis of Social Structure." This is worth more than everything else in the book put together. Part IV of the volume, which deals with "The Potentialities of the Child," being a little more positive than the rest, contains some interesting contributions, and the paper by Havelock Ellis on "Perversion in Childhood and Adolescence," though it contains little or nothing that is new, is a valuable study. It is a great pity that Malinowski's essay cannot be taken out of this setting and printed separately. Most of the rest could be left without regret to whatever fate awaits it.

Gestalt Psychology and Meaning. By W. D. Ellis. Berkeley: The Sather Gate Book Shop. 1930. Pp. xi + 172.

In spite of a certain—perhaps inevitable—obscurity of terminology, and a certain jumpiness of style which seems odd in an adherent of the psychology known as 'Gestalt,' this is a very good essay. The author is thoroughly conversant with the work of the school whose views primarily concern him, and has also read widely in general psychology. Moreover he is distinctly less intolerant in his attitude to more analytic methods than many recent writers have been. Whether he clears up the intractable problems of meaning or not he certainly gives a fair and full account of many interesting experiments. The first part of the essay is general and considers Elementaristic Psychology, Teleological Psychology, and 'Gestalt' Psychology. The second part deals fairly concretely with specific problems of meaning. The third part discusses and abundantly illustrates 'Gestalt' interpretations of certain phenomenological data, and the fourth part chiefly discusses verbal and mental behaviour and the relation of meaning to other problems. The Lexicon which forms Appendix A will be particularly welcome to many students. Appendix B concerns the Philosophy of 'Gestalten.' The whole essay is rather speculative in bias but is, nevertheless, a very able piece of work.

Ethical Problems; An Introduction to Ethics for Hospital Nurses and Social Workers. By Beatrice Edgell. London: Methuen & Co. 1929. Pp. ix + 149. 5*s*. net.

Although this book is addressed specifically to hospital nurses and social workers it could be read and studied with great advantage to themselves by all students, within college or without, who are approaching the study of ethics. The main reason for this is that Prof. Edgell arrives at her ethical problems by a study of the basic character and determinants of human action. Consequently her discussions have an air of reality which is in welcome contrast to the word-spinning of many widely used ethical text-books. In Part I, which concerns 'principles,' several of the traditional ethical problems are considered, always with a most careful definition of the terms used. Part II concerns 'practical applications,' and is full of acute psychological observation. Although the book is very conspicuously clear both in style and in matter, it is not popular in the sense that it can be read lightly and thoughtlessly. It does indeed excellently fulfil its purpose.

Introduction to Mental Hygiene. By E. R. Groves and Phyllis Blanchard. London: Gerald Howe, Ltd. Pp. vi + 467. 16*s*. net.

This is a general text-book dealing with the healthy regulation of life from childhood, through adolescence to the adult stage in many different spheres. Each chapter contains a discussion, in somewhat general terms, of some special problem, such as Mental Disease, Mental Hygiene and Marriage, Mental Hygiene and Business, and is followed by suggestions for class-room discussions and for written work. A selected Bibliography, as a rule rather too full, is added to each discussion. The writing is uniformly clear, generally very sensible and is well informed. As an introductory work the book may be pronounced a success.

The Measurement of Man. By J. A. Harris, C. M. Jackson, D. G. Paterson, and Richard E. Scammon. University of Minnesota Press. 1930. Pp. vii + 215. $2.50.

J. A. Harris writes on "The Measurement of Man in the Mass"; C. M. Jackson on "Normal and Abnormal Human Types"; D. G. Paterson on "Personality and Physique"; R. E. Scammon on "The Measurement of the Body in Childhood." The scope of the book is accurately indicated by these titles. Each essay gives a mass of carefully collected and sifted information and succeeds in presenting the data both clearly and in an interesting manner. Moreover there is incorporated a considerable amount of original observation. The whole book is a valuable contribution to human science, and deserves a cordial welcome.

Academic Prognosis in the University. By H. A. Egerton. Baltimore: Warwick & York. 1920. Pp. vii + 83. $1.88.

This volume is No. 27 in Educational Psychology Monographs. The author explains a scheme of progressive prognosis to be applied to University students, beginning with Intelligence Tests on entry and continuing by the use of combined results of intelligence tests and achievement performances throughout a student's University career. Considerable and increasing accuracy of prediction is indicated, and it is claimed also that the method may have considerable bearing upon personality assessments.

Educational Achievement in Relation to Intelligence. By C. W. St John. Harvard Studies in Education, vol. 15. Cambridge: Harvard University Press. 1930. (London: Humphrey Milford.) Pp. xiv + 219. 16*s.* net.

The general problem is "What is the precise relation between ability as indicated by *general* experience and achievement and school abilities?" An extensive survey reveals a fairly close agreement between the two with, however, considerable discrepancies. These discrepancies are closely studied and an attempt is made to trace their causation and significance. The whole work is very thorough and issues in many interesting suggestions both of theoretical and of practical import. Eight pages of classified bibliography follow the argument of this clearly designed and carefully executed piece of research.

The Modern Parent. By G. C. Myers. London: Williams & Norgate, Ltd. 1930. Pp. xiii + 350. 8*s.* 6*d.* net.

If there is safety in a multitude of counsellors the modern parent should be in a very happy position. At any rate, whatever he does he is now fairly certain to be able to shelter himself behind somebody or other who has written in the name of psychology to say that this particular procedure is a sound one. These remarks are not intended to cast any unpleasant aspersions upon the book under notice, which is in fact a very clear, sane and level-headed one. The parent may be horrified to learn what a very large number of unfortunate predicaments he may ignorantly land his children into, but at least some excellent advice is given him by following which he may reasonably hope to avoid the worst. Often somewhat vague, sometimes sententious, occasionally ponderous, the book certainly manages to avoid undue technicality. If any criticism is called for, the most obvious point to make is that the author is more emphatic and expansive on what must not be done than upon what should positively be attempted.

Religion and the Reign of Science. By F. L. Cross. London: Longmans, Green & Co. 1930. Pp. 110. 4*s.* net.

One chapter in this book deals with Psychology and Religion. This chapter is not very well-informed and is very limited in its outlook. The author's bias is towards philosophy, and the only psychological work he considers even in scanty detail is that of the behaviourists, that of the psycho-analysts, and some of the work of people interested in psychical research. Concerning this nothing new is said. However, the rest of the book which deals with other departments of science and knowledge may be upon a higher plane.

Guiding the Child. By Alfred Adler and his Associates. Translated from the German by B. Ginzburg. London: George Allen & Unwin, Ltd. 1930. Pp. viii + 268. 10*s.* 6*d.* net.

The bulk of this book is the work of the 'Associates.' Adler himself contributes one short essay on "A Case from Guidance Practice." Most of the discussions are based, either directly or indirectly, upon case history studies in the Child Guidance Clinics of Vienna. A considerable amount of useful illustrative material is presented. In general the discussions are untechnical, intended for the general reader, and rather brief.

Mind at the Crossways. By C. Lloyd Morgan. London: Williams & Norgate. 1929. Pp. xi + 275. 10*s.* 6*d.* net.

This book is concerned in the main with an interpretation of nature and not with the determination of human reactions or with a study of their genetic significance in human development. Consequently its bias is philosophical throughout, and though many psychological problems are discussed, they are considered mainly in reference to the bearing which they are said to have upon doctrines of 'emergent evolution.' Mind is said to 'stand' somewhere "at the emergent passage from percipience to perception," or, again, "at the emergent switchpoint of divergence from the ascending line of perception to a new and higher line of ascent which comes on the scene with the advent of reflection." It is also thought that there may be Mind at every new departure of natural development, though not within every such departure, and that *this* mind may be called 'Divine Purpose.' Taking it altogether the book is not as concise as it might be, or as clear; and though, as always, Prof. Lloyd Morgan has many striking and attractive things to say, he lets his bent for simile and metaphor have somewhat too free a fling. A psychologist will often find himself labouring far behind the course of argument whatever he may feel inclined to think of the conclusions.

Our Knowledge of Other Minds. By W. W. Spencer. New Haven: Yale University Press. 1930. (London: Humphrey Milford.) Pp. 145. 9*s.* net.

There are three kinds of knowledge: direct experience; inference or induction from particular facts; and knowledge based upon practical adaptations taking place in a social community. Our knowledge of other minds is of the third kind, but need not lack assurance on that account. The position is ably argued, the author's main bent being philosophical.

The Revolt against Dualism. By Arthur O. Lovejoy. London: Grant Allen & Unwin, Ltd. 1930. Pp. xii + 325. 15*s.* net.

Spirit. By T. A. Bowhay. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1930. Pp. viii + 224. 5*s.* net.

Songs of the Soul. By Aura May Hollen. Hollywood, California: Keat's Publications. 1930. Pp. 95.

A Philosophy of Reality. By E. L. Young. Manchester University Press. 1930. Pp. xi + 266. 8*s.* 6*d.* net.

COMMITTEE FOR RESEARCH IN EDUCATION.

With a view to facilitating the general co-ordination of research in education, the Committee has made its annual enquiry as to researches in progress, or planned, at the various Psychological Laboratories and Training Departments.

The following is a summary of the replies received:

RESEARCHES IN PROGRESS OR PLANNED, DECEMBER 1930.

BINGLEY TRAINING COLLEGE.

The intelligence and educational abilities of physically defective children.

UNIVERSITY OF BIRMINGHAM.

A statistical enquiry as to the correlation of school and university successes.
Tests of higher mental processes.
Studies in the psychology of early childhood.
Reasons for the choice of occupation among secondary school pupils.
An experimental enquiry into the suitability of the Otis group tests of intelligence for use with matriculated students.
The development of humour in children.
A psychological study of Francis Bacon.
Analysis of examination marks with a view to methods of standardization and adjustments between different training colleges.
Preference of children and adults in poetry and prose literature.
Errors in English composition as revealed in essays written by Higher School Certificate candidates and University post-graduate students.

UNIVERSITY OF DURHAM. Armstrong College.

The development of memory in its relation to other mental functions.
An investigation into the variation of perseveration among school children and its effect upon school attainments.

UNIVERSITY OF EDINBURGH. George Combe Psychological Laboratory.

Group tests of colour vision for use in schools.
The motivation and accessory concomitants of delinquency, with special regard to adolescents.
The speed factor in intelligence.
Variations in suggestibility with age, sex, and grades of intelligence.
A psychological study of art with special reference to pictorial art.
A psychological investigation of the perception and memory of time relations and the development of time concepts in high school pupils.

UNIVERSITY OF EDINBURGH. Education Department.

A study of "answer-patterns" in tests.
Inquiries into "absolute" scale values for test-items.
Comparison of broadcast, gramophone, silent reading, and oral lesson.
Measurement of the value of oral composition as a teaching method.
Binet tests in two Indian languages.
The improvement of methods and materials in native African schools.
Mathematical enquiries into problems of sampling.
Factors in teaching, learning and testing spelling.
Follow up enquiries of children of known intelligence.

LONDON DAY TRAINING COLLEGE.

The development of aesthetic appreciation in children.
Collection of data for re-standardization of Merrill Palmer performance tests.
Psychological problems in the teaching of geography, with special reference to sex-differences.
Vocational guidance in secondary schools.
Treatment of the dull and backward in elementary schools.
Personality traits and persistence.

UNIVERSITY OF LONDON. Bedford College.

Children's thinking.
Study of the part played by imagery in learning (continuation).
Intelligence in relation to the social environment.
Eyedness and handedness.

UNIVERSITY OF LONDON. King's College.

The perception of tachistoscopically exposed words.
Frustration of will acts and conation.
Perseverance and perseveration.
Influence of affection and will on ergographic performance.
Psychology of evidence. Comparison of testimony in normal and delinquent youths.

UNIVERSITY OF LONDON. University College.

Function of images.
Intelligence of three-year old children.
Comparative value of verbal and perceptual tests.
Ability to reason.
Measurement of the "*g*" of the blind.
Training the imagination.
Tests of musical ability.
Tests of oscillation.
Psychological changes in adolescence.
Oral and written tests of "*g*."
Formal training in arithmetic.
Tests of arithmetical ability.

UNIVERSITY OF MANCHESTER.

Methods of provoking mental imagery.
Psychological problems of radio-drama.
Investigations of auditory eidetic imagery.
The relative economy of distributed and concentrated practice time in learning manual skills.
The effect upon the efficiency of localization of sound of certain factors, including frequency, auditory fatigue and directional adaptation.

NATIONAL INSTITUTE OF INDUSTRIAL PSYCHOLOGY.

The use of psychological tests in the vocational guidance of elementary school children in Fife and Birmingham.
The use of psychological tests in allotting boys to working parties in Borstal Institutions.
The measurement of perseveration and the relation of this quality to introversion, extraversion and leadership.
The nature and measurement of the abilities involved in "intelligent" and "routine" assembling.
The nature and measurement of "social ability."
The devising and standardization of tests for motor drivers.
The devising and standardization of tests for colour-discrimination.
Occupation analysis: analysis of the duties involved and psychological qualities required in professional and higher commercial occupations.
Study of the occupational prospects for boys in Tottenham.

UNIVERSITY COLLEGE, NOTTINGHAM.

The relation between literary expression and personality in children.
Children's comprehension of written language.
The development of language in children from five to eight years of age.

UNIVERSITY OF SHEFFIELD.

The development of reasoning in children.
Studies in children's vocabularies.
An investigation of the influence of immediately antecedent activity on the learning ability of elementary school children between the ages of ten and eleven years.

UNIVERSITY OF SOUTH WALES. CARDIFF.

Variations in the development of normal adolescents.
The rhythm factor in literary appreciation.
The measurement of the intelligence of monolingual and bilingual children.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

GENERAL MEETINGS.

December 13, 1930. "Thinking as an Instinct." By Professor Heidbreeder (U.S.A.).

"Application of the Theory of Two Factors to Non-verbal Tests." By Dr Stephenson.

"Perceptual Tests of 'G'." By Dr Fortes.

"Demonstration of some new Tests of Musical Ability." By Mr Drake.

SECTIONAL MEETINGS.

Medical.

November 26, 1930. "Social Behaviour of the Primates." By Dr S. Zuckerman.

December 17, 1930. "The History of the Conception of 'Nervous Disorder'." By Dr Millais Culpin.

January 14, 1931. A paper by Dr Alfred Adler.

Education.

December 1, 1930. "Some Case Studies of Delinquent Girls Described as Leaders." (From Social Study made in the County of Los Angeles, California.) By Miss S. Clement Brown, M.A.

December 31, 1930. "Learning how to Study." By Professor T. H. Pear.

February 2, 1931. "Sublimation: A Correlation between the Experiences of an Educator and Psycho-Analyst." By Miss Ella Freeman Sharpe.

Industrial.

December 18, 1930. "Incentives." By Mr L. I. Hunt, B.A.

Aesthetics Section.

November 14, 1930. "An Attempt at Representing the Rhythms of Poetry Accurately." By Miss K. M. Wilson, Ph.D.

December 10, 1930. "Aesthetics and Philosophy." By Miss L. S. Stebbing.

SCOTTISH BRANCH.

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Edinburgh.

November 1, 1930. "Psychology and Social Problems." By Dr Shepherd Dawson.

"Demonstration of Phono-Projectoscope." By Dr Drever.

"'Instinct' as an explanatory psychological concept." By Mr C. A. Mace.

February 7, 1931. "The human factor in production." By J. A. Fraser.

"A new performance test of intelligence." By Mr Alexander.

"The speed factor in intelligent reactions." By J. D. Sutherland.

Glasgow.

December 6, 1930. "The Weber-Fechner Law." By Dr Houston.

"Weber's Law." By Dr R. H. Thouless.

"The analogy between mental images and sparks." By Dr L. F. Richardson.

March 7, 1931. "The development of intelligence in children after Encephalitis Lethargica." By Mr J. C. Conn.

"The survival value of intelligence." By Dr W. R. D. Fairbairn.

"A test of musical appreciation." By Mr Macfarlane.

GERMAN PSYCHOLOGICAL SOCIETY

The XIIth Congress of the German Psychological Society will be held at Hamburg from 12*th*–16*th April* 1931. *Further information may be obtained from Prof. W. Stern, Hamburg.*

BRITISH PSYCHOLOGICAL SOCIETY

REVENUE ACCOUNT AND BALANCE SHEET AT 30 SEPTEMBER 1930

Revenue Account for the year ended 30 September 1930

Year to 30 Sept. 1929			Expenditure						Year to 30 Sept. 1929		
£	s.	d.		£	s.	d.	£	s.	d.		
50	0	0	*To* Rent				50	0	0		
79	6	6	Printing				73	16	10		
37	15	9	Hire of Halls				26	13	6		
			Office and General Expenses:								
			Secretary	200	0	0					
			Addressing	16	3	6					
			Stationery	8	2	4					
			Postage and Telephone...	34	6	0					
			Travelling Expenses ...	5	10	8					
			Subscriptions to outside Associations	6	16	6					
			Audit Fee 1929	15	15	0					
			Sundry Expenses ...	11	12	1					
300	5	1					298	6	1		
262	2	2	Journals Publication Expenses				229	14	0		
11	3	9	Teas...				8	13	5		
8	0	0	Scottish Branch Expenses				7	18	1		
2	12	6	Midland Branch Expenses					5	0		
	10	0	Bank Charges					8	7		
—	—	—	Committee for Research in Education ...				30	0	0		
48	13	11	Library Depreciation				46	2	10		
800	9	8					771	18	4		
337	15	2	Balance, being Surplus of Income over Expenditure				386	13	3		
£1138	4	10					£1158	11	7	£1158	11	7	£1138	4	10

	Income						Year to 30 Sept. 1929		
	£	s.	d.	£	s.	d.	£	s.	d.
By Subscriptions 1930	1184	10	0						
1929	3	8	0						
	1187	18	0						
Less,									
Resignations and Removals	129	17	0						
				1058	1	0	1047	0	6
Midland Branch (2 Associate Members) ...					5	0		7	6
Interest on Deposit Account				12	12	9	2	8	10
Interest on Investments				87	12	10	88	8	0

£ s. d.		£ s. d.	£ s. d.
	Sundry Creditors		
	Rent	12 10 0	
	Office and General Expenses...	16 4 10	
	Journals Publication Expenses	16 18 6	
	Scottish Branch Expenses ...	7 18 1	
	Library Expenditure	18 2 9	
22 15 0			71 14 2
50 14 6	*Subscriptions in Advance* ...	... ...	30 17 0
	Publications Reserve		
	Balance as per last Balance		
172 0 0	Sheet	172 0 0	
	Less, Grant made during year	50 0 0	
			122 0 0
	Revenue Account		
	Balance as per last Balance		
	Sheet	3424 0 1	
	Add, Surplus of year	386 13 3	
3424 0 1			3810 13 4
£3669 9 7			£4035 4 6

£ s. d.	£ s. d.	£ s. d.	£ s. d.
Cash at Barclays Bank Ltd.			
Deposit Account	400 0 0		
Current Account	63 5 5		
		463 5 5	435 5 1
Postage Stamps in Hand		3 4 5	— — —
Investments (at cost)			
£100 New South Wales 5½°/₀			
Loan 1924–34	100 0 0		
£1000 4°/₀ War Loan 1929–42	1013 5 6		
£1100 5°/₀ War Loan 1929–47	1117 6 9		
£300 5°/₀ Conversion Loan			
1944–64	318 9 9		
	2549 2 0		
Note. Market value of above securities at 30 Sept. 1930 £2560. 13*s.* 9*d.*			
500 National Savings Certificates (dated 20 Feb. 1923)	400 0 0		
		2949 2 0	2630 12 3
Subscriptions in arrear ...	... ...	204 7 0	165 6 6
Library			
Balance as per last Balance Sheet	438 5 9		
Expenditure during year ...	23 2 9		
	461 8 6		
Less, Depreciation	46 2 10		
		415 5 8	438 5 9
(Of the Expenditure on Library of £150 authorised on 8 March 1928 the sum of £134. 0*s.* 6*d.* has been spent to date leaving a balance of £15. 19*s.* 6*d.* unexpended.)			
		£4035 4 6	£3669 9 7

Note. Stock of Journals. There exists a number of past and present volumes of Journals in Stock, but owing to the possibility that a large proportion of these may never be sold, no valuation can be placed on such stock.

I beg to report that I have examined the foregoing Accounts with the books and vouchers of the British Psychological Society and that I have obtained all the information and explanations I have required. In my opinion the Balance Sheet is properly drawn up so as to exhibit a true and correct view of the state of the Society's affairs according to the best of my information and the explanations given to me and as shewn by the books of the Society. Investments, which are lodged with the Bank, have been verified by production of Banker's Certificate.

T. BARRETT,
Incorporated Accountant.

63, Eastcote Lane,
Harrow.
12 *November* 1930.

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BRITISH PSYCHOLOGICAL SOCIETY

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BRITISH PSYCHOLOGICAL SOCIETY

RULES

NAME AND OBJECTS.

1. This Society shall be called the British Psychological Society.

2. The Society is instituted for promoting the co-operation of those interested in the various branches of Psychology.

3. The Society shall be responsible for the publication of *The British Journal of Psychology*.

MEMBERS AND SECTIONS.

4. The Society shall consist of Honorary Members, Ordinary Members and Sectional Associates.

5. Persons of scientific distinction who have contributed to the advancement of Psychology shall be eligible for election as Honorary Members.

6. Honorary Members shall be entitled to all rights and privileges of membership.

7. Honorary Members may be elected from time to time on the nomination of the Council at any General Meeting of the Society.

8. The Council shall be empowered to institute Sections of the Society, each Section being concerned with a special branch or aspect of Psychology.

9. Election to a Section or Sections shall *ipso facto* constitute election to the Society as a whole and entitle those so elected to receive notice of and to take part in all General Meetings of the Society in addition to the meetings of the Section or Sections to which they belong.

10. Sections shall have the right of electing Sectional Associates who shall receive notice of and shall be permitted to attend all Meetings of the Section or Sections to which they belong. Such Sectional Associates shall however take no part in the Government of the Society or the Sections, and they shall not receive *The British Journal of Psychology*.

10 (*a*). Transfer from Sectional Membership to Sectional Associateship may take place on January 1st only, and after not less than six months' notice from the Member of his wish to transfer.

11. The procedure as regards the election of Sectional Associates shall be the same as that in the case of Members.

12. Every Candidate for election to the Society shall be recommended by at least two Members who shall be prepared to furnish information as to the Candidate's qualifications for membership. No Candidate shall be submitted for election to the Society who has not first been approved by the Council or by a Sectional Committee.

13. A Candidate who has been approved by the Council or by a Sectional Committee shall be nominated for election at the next General Meeting of the Society, or at the next Meeting of the Section respectively.

14. One full week before the Meeting at which the ballot is to take place, the Secretary shall send to each Member a balloting paper containing the names and addresses of the Candidates and the names of their proposers.

15. Any Member unable to attend a Meeting at which a ballot is to take place can vote by sending his balloting paper to the Secretary.

16. One adverse vote in five shall exclude.

17. To vote *for* a Candidate, a Member must prefix a cross to the Candidate's name; to vote *against* a Candidate, he must erase the Candidate's name. Members who wish to record a vote neither for nor against a Candidate must leave the name untouched.

18. Any Member whose subscription remains unpaid for two years may, after receiving due notice, be removed by a resolution of the Council from membership of the Society.

19. Whenever it shall be proposed to remove a Member from the Society for any reason other than failure in payment of subscription, the matter shall be brought as a formal resolution before the Council or one of the Sectional Committees (whichever it shall most concern). In the event of the resolution being passed, a ballot shall be held for the removal of the said Member at the next General Meeting of the Society, or at the next meeting of the Section respectively. Should two-thirds of the Members voting vote in favour of the removal of the Member, that Member shall be removed from the Society.

20. If at any time it is desired to abolish any Section or Sections, to subdivide any Section or Sections, or to amalgamate two or more Sections, a Special Meeting of the Section or Sections concerned may be called for this purpose according to the conditions laid down in Rule 44. Resolutions passed at any such Meeting shall be reported by the Sectional Secretary or Secretaries concerned to the next Meeting of the Council. Should the Council approve of such resolutions, such resolutions shall immediately take effect. But should the Council formally express dis-

approval of any or all of the said resolutions, these resolutions shall be considered by an Extraordinary General Meeting of the Society summoned in accordance with the conditions laid down in Rule 44 and the decision of this Meeting shall be final.

In the case of the desire to abolish any Section or Sections, the following alternative procedure may be followed. An Extraordinary General Meeting of the Society shall be summoned in accordance with the conditions laid down in Rule 44, and any resolution passed at this Meeting regarding the abolition of the said Section or Sections shall be final.

OFFICERS, COUNCIL AND SECTIONAL COMMITTEES.

21. The business of the Society as a whole shall be conducted by a Council consisting of the President, Vice-Presidents, Treasurer, General Secretary, Librarian, Editors of *The British Journal of Psychology*, one representative each of every Section and Branch to be nominated by the Sectional and Branch Committees, one representative of the Committee for Research in Education to be nominated by that Committee, and of six Ordinary Members.

22. The President of the Society shall hold office for three years and shall not be eligible for re-election until after the lapse of a further period of six years. Every President shall at the termination of his period of office become a Vice-President for a period of six years. The other officers of the Society shall retire annually but shall be eligible for re-election.

23. Every year two ordinary members of the Council, having served for three years, shall retire and shall be ineligible for re-election until after the lapse of one year.

24. Names of Members to serve on the Council may be suggested by the Council or by any Member by sending the proposed names to the Secretary at least two weeks before the Annual General Meeting.

25. Nominations for the election of Officers shall be handed in to the General Secretary before the Meeting preceding the Annual General Meeting and be announced at the Meeting preceding the Annual General Meeting.

26. One full week before the Annual General Meeting, the General Secretary shall send to each Member a balloting list containing: (1) The names of the Council; (2) The number of the attendance of members of the Council at the Council Meetings held during the year; (3) The names of those proposed by the Council or by Ordinary Members to fill the vacancies.

27. The Society may fill all the vacancies among the officers that may occur between two Annual General Meetings, by ballot in accordance with Rule 15 at the next General Meeting, due notice having been given. Similar vacancies among the ordinary members of the Council shall be filled by co-option by the Council.

28. In the absence of an ex-officio Chairman (in accordance with the conditions laid down in Rule 42) the Council shall elect one of its Members to act as Chairman of Council. A quorum at a Council Meeting shall consist of five. If there be no quorum, the Members attending shall have power to transact business, subject to the approval of absent Members, to whom the resolutions adopted shall be forwarded.

29. The first Meeting of each Special Section shall be convened by the General Secretary of the Society. At this Meeting the Section shall appoint its Chairman, Secretary and Sectional Committee.

30. The Chairman and Secretary of a Section shall retire annually but shall be eligible for re-election.

31. The size of a Sectional Committee shall be determined by the Section. A quorum at a Meeting of a Sectional Committee shall be as that Committee shall decide.

32. When Sections have been formed, Rules 12, 13, 14, 15, 23, 24, 25, 26, 27, 28, 40, 41, 42, 43, 44 shall (*mutatis mutandis* and unless expressly stated to the contrary) apply respectively to the Committees, Chairmen and Secretaries appointed by these Sections.

33. The Society shall not make any dividend gift, division or bonus in money unto or between any of its Members.

SUBSCRIPTIONS AND FINANCE.

34. Each Ordinary Member shall pay an annual subscription of one guinea, which shall entitle him to receive a copy of *The British Journal of Psychology*, with the exception of such additional portions of the *Journal* as are devoted to special branches or aspects of Psychology and are published separately.

34 (*a*). Ordinary Members who are members of all Sections shall pay an inclusive annual subscription of £2. 12*s*.

35. In addition to the Ordinary Membership Subscription Members shall pay an additional fee for every Section which they join. This additional fee shall entitle Members of any Section to receive such additional, and separately published portions of *The British Journal of Psychology* as may be devoted to the special branch or aspect of Psy-

chology studied by the Section. The amount of such additional fee shall be fixed by the Committee of the Section concerned in consultation with the Treasurer, and any resolution passed by the Sectional Committee with reference to such fee shall, before coming into force, receive the approval of the Council.

36. Sectional Associates shall pay a subscription the amount of which shall be determined by the Sectional Committee in consultation with the Treasurer of the Society.

37. A Member may compound for his annual subscription by a fee, the amount of which shall be determined by the Council after consultation with the Sectional Committee and the Treasurer. Such composition shall entitle the Member to all the privileges of membership, which he enjoys at the time of composition, for life.

38. The subscription is payable in advance and becomes due on January 1st. *The British Journal of Psychology* shall be sent only to Members whose subscriptions have been duly paid.

39. The Council shall grant to each Section permission to incur such expenses as may be necessary for the work of the Section; the approximate amount of such expenses to be determined by the Council.

MEETINGS.

40. The Annual General Meeting shall be held in December, when the General Secretary's Report and the Treasurer's Statement of Accounts shall be laid before the Society.

41. The General Meetings of the Society shall be at such times and places as the Council shall decide, but in each year there shall be at least four meetings.

42. The President, or, failing him, a Vice-President, shall, whenever possible, act as Chairman at the Meetings of the Council and of the Society. In the absence of the President or a Vice-President, some other Member shall, on the motion of the General Secretary or his substitute, be elected to act as Chairman.

43. The Secretary shall send to all Members of the Society notices of each Meeting at least seven days beforehand, and of the business to be transacted at the Meeting.

44. An Extraordinary General Meeting may be summoned at any time by the Council, or by the General Secretary on the written request of twenty Members; at least fourteen days' notice of such Meeting shall be sent to each Member of the Society.

VISITORS.

45. Each Member shall have the privilege of introducing two visitors at any Meeting. The Council or a Sectional Committee shall however have the power of closing any Meeting or Meetings to visitors, provided notice to this effect be given on the notice convening such Meeting or Meetings. The names of visitors together with the names of Members introducing them shall be entered in a book provided for that purpose. Any Member of the Society attending a Meeting of a Section to which he does not belong must be introduced by a Member of that Section.

BRANCHES.

46. The Council shall be empowered to sanction and dissolve the establishment of Branches of the Society throughout the British Empire. Members of such Branches shall pay the same subscription as that of Ordinary Members direct to the Treasurer of the Society and be entitled to the same rights and subject to the same rules as Ordinary Members. The Council shall be empowered to refund to the Treasurer of any Branch such proportion of Branch Members' subscriptions as the Council may determine to meet local expenses.

46 *a*. Any Rules made by Branches of the Society shall be first submitted to the Council for their approval.

46 *b*. The minimum number of Members necessary to form a Branch shall be 10.

46 *c*. Branches shall be required to present a report to each Annual General Meeting.

RULES.

47. The Rules of the Society may be altered at the Annual General Meeting, after notice of alteration has been given at a previous Ordinary General Meeting, or they may be altered at an Extraordinary General Meeting summoned for that purpose. Such Extraordinary General Meeting shall be held in London, and the purpose for which it is called shall be expressly stated in the notice convening the Meeting. No other business shall be transacted thereat.